

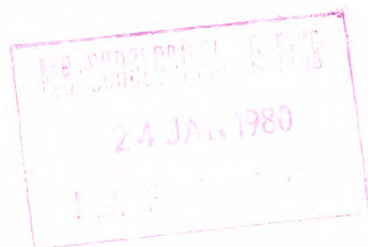


# THE METEOROLOGICAL MAGAZINE

HER MAJESTY'S  
STATIONERY  
OFFICE

January 1980

Met.O. 931 No. 1290 Vol. 109



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# THE METEOROLOGICAL MAGAZINE

No. 1290, January 1980, Vol. 109

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## **Examples of banded rainfall distributions in potentially unstable conditions over southern England**

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### **Summary**

Maps of 24-hour rainfall totals during the period October 1977 to February 1978 have been examined with special regard to spatial variability of rainfall. Variability was greatest in unstable situations and marked rainbands, similar to those previously documented in warm-sector or pre-warm-sector conditions, occurred in several potentially unstable situations. It is suggested that the main factors involved in the formation of such rainfall distributions include orographic influences, variability of surface (land and sea) temperatures, localized low-level convergence and, on a larger scale, the effects of low-level troughs.

### **Introduction**

Nicholass and Harrold (1975) showed that, if  $R_s$  is the rainfall in a stated period averaged over a subcatchment of area, say, 60 km<sup>2</sup>, and if  $R$  is the rainfall in the same period averaged over a much larger area containing the subcatchment, the ratio  $R_s/R$  is dependent upon synoptic type and upon surface wind direction. It was found, generally, that  $R_s/R$  varied between about 0.5 and 2.0 and that the distribution of areal rainfall for particular synoptic types could be predicted, provided that the large-scale rainfall could be forecast perfectly over the whole catchment. However, the largest scatter of  $R_s/R$  was, not surprisingly, found to be in synoptic types giving showers and thunderstorms where, in one subcatchment, the ratio fell to 0.03.

Maps of 24-hour daily rainfall totals (09–09 GMT), plotted by computer and analysed by meteorologists, have been available as aids to the quality control of rainfall totals since October 1977, and a record has been kept of those charts displaying coherent isohyetal patterns of highly variable precipitation. From about 250 maps available for study, there were 34 which would appear to justify further investigation. Fourteen showed marked rainbands\* and, of these, 7 were on days with broadly similar synoptic situations and characterized by sporadic outbreaks of rain or by showers. Circumstances

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\* A rainband is regarded as a distribution of rainfall shown by an isohyetal pattern with a long narrow area of high rainfall.

associated with these situations were examined, employing such information as would be available to a bench forecaster, in an endeavour to identify, in a qualitative way, possible associations with the appearance of the rainbands.

The rainband phenomenon has been studied in detail by Harrold (1973), Browning, Hardman, Harrold and Pardoe (1973), Browning and Bryant (1975), and Hobbs and Locatelli (1978), and is well documented. Harrold (1973) paid much attention to the 'conveyor-belt' as an important mechanism in the large-scale distribution of precipitation. He described the conveyor-belt as a 'well-defined stream of air, bounded at the top by air of a different origin advecting over the cold front, bounded in the west by the cold front and in the east by the edge of the significant northward flow of air'. Later, he stated that 'the effect which orography has on the precipitation within a baroclinic disturbance depends upon the stability of the atmosphere, the effects being more marked in an unstable atmosphere, that is toward the western warm-sector portion of the conveyor-belt'. The example he quoted, with a 24-hour rainfall total map for the period ending 09 GMT on 12 November 1969, was indeed most striking. It showed a rainband extending from the Brecon Beacons, north-eastwards, right across to the north-east coast of England, east of the Yorkshire Wolds. Almost all this rain, however, fell within a warm sector, whereas, in the examples which follow, the rainbands occur in showery, cyclonic situations and are again notable for their large gradients of rainfall depth.

### Synoptic situations

#### (a) 5–9 October 1977

A complex area of low pressure covered western parts of the British Isles throughout the period (Figures 1(a), 2(a), 3(a), 4(a)) with an associated upper vortex (Figures 1(b), 2(b), 3(b), 4(b)). As shown by the Camborne upper-air ascents (Figures 1(c), 2(c), 3(c), 4(c)), the air mass remained potentially unstable to at least 3500 m. Low 'D' intensified during the 5th with the main centre forming just off the Brest Peninsula and deepened to 984 mb by 06 GMT on the 6th. During the 6th a number of fronts developed and moved northwards over the British Isles (Figures 1(a), 2(a)). By 06 GMT on the 7th the whole of southern England was covered by cold air and was later affected by a series of troughs (Figure 4(a)); this situation persisted until the 9th. Winds at 700 mb were generally southerly over southern England throughout the period, backing temporarily south-easterly on the 7th and finally veering south-westerly by 00 GMT on the 9th.

*Discussion.* Shone (1979) has discussed the heavy rainfall over west Cornwall during the period 18 GMT on 5 October to 12 GMT on 6 October 1977 and suggested that, in common with a similar occurrence of heavy rain on 21 March 1976 (Shone 1978), backing and strengthening of the thermal wind and hence increasing baroclinicity was a significant contributory factor. The heaviest rain fell in a narrow band just west of the surface trough (Figure 1(a)) which travelled only a short distance east from St Mawgan and Cudrose before halting and returning westward. It is worthy of note that one rainfall station within this rainband reported 94 mm of rain during the 24-hour period ending at 09 GMT on 6 October, whilst another station, only 13 km to the east, reported only 11 mm (Figure 1(d)).

It would seem that, in addition to the strengthening of the baroclinic zone, marked low-level cyclonic wind shear on the surface trough could have contributed to the trigger required to release the potential instability shown by the Camborne ascent (Figure 1(c)), the rainband being aligned with the 700 mb wind (Figure 1(b)).

By 00 GMT on 7 October a separate upper vortex had developed in association with Low 'D', causing the 700 mb winds to back to south-easterly over south-west England (Figure 2(b)). Although the two cold fronts (Figure 2(a)) moved north-eastwards across the study area, the rainfall distribution



(Figure 2(d)) still had a banded structure with the bands aligned with the 700 mb wind direction. Some apparent orographic enhancement was also in evidence over the higher ground of Dartmoor and Exmoor. At midnight on 8 October there were no fronts over England or Wales (Figure 3(a)) but the low-level north-west-moving air mass remained potentially unstable (Figure 3(c)), requiring a temperature of around 16 °C to generate convection leading to showers. Figure 3(e) shows the five-day mean sea isotherms for 5–9 October for the English Channel. It can be seen that mean sea temperatures in the vicinity of the Isle of Wight were likely to have been generally about 16 °C; maxima of 17 °C were reported, high enough to initiate convection leading to shower formation. Once these convective cells had developed, they moved northwards in the southerly upper flow (cf. 700 mb wind flow, Figure 3(b)). Figure 3(d) shows that the rainfall distribution for the 24-hour rainfall period ending at 09 GMT on 8 October had two distinct bands of higher rainfall. The apparent association with the shallow Weymouth and Swanage Bays, both probably having sea temperatures a little higher than those off St Alban's Head, gives grounds for speculating that the rain-producing clouds were generated there.

The 12 GMT chart for 8 October (Figure 4(a)) shows a trough-line, separating low-level southeasterly winds from south-westerlies, moving up towards southern England from Brittany. This trough was responsible for fairly widespread outbreaks of rain over central southern England. Figure 4(d) shows, however, that the rainfall distribution still retained a definite rainband pattern: one rather wide band extending north-north-eastwards from the west Solent and further narrower bands extending north-eastwards, one aligned with the 700 mb flow (Figure 4(b)) from the South Downs, north of Selsey Bill, and another from near Bexhill. It would seem that potential instability (see Figure 4(c)) may have been released as the low-level flow was lifted over the South Downs which rise to 200 m in places; stations south of the Downs all reported less than 10 mm of rain during the 24-hour period ending at 09 GMT on 9 October. Further west, the higher inshore sea temperatures may have been a contributory factor to convection, as on the previous day.

#### (b) 20–22 October 1977

The synoptic situation described is almost identical with that for 25 February 1978. On both occasions a depression remained almost stationary west of Ireland, near 20°W, with a warm southerly airstream covering the British Isles (Figures 5(a), 6(a)). The Crawley upper-air ascents (Figures 5(c), 6(c)) indicate that the air masses were potentially unstable, requiring only a trigger to set off convective activity. The 700 mb winds (Figures 5(b), 6(b)) were steady from 200–210 degrees on the east side of an upper vortex or trough associated with the surface depression.

*Discussion.* The remarkable rainfall total distribution shown by Figure 5(d) is not apparently associated with marked frontal activity (Figure 5(a)). The five-day mean sea isotherm chart (Figure 5(e)), covering the period, indicates a warm area, with maxima of 17 °C reported over the central Channel. This is the surface temperature value required to release the potential instability shown by the 00 GMT Crawley upper-air ascent (Figure 5(c)) for 21 October. Convective cells so generated would have moved with the upper-wind fields (of which the 700 mb chart (Figure 5(b)) is indicative), the band of rain so produced growing narrower and dying out some 150 km (2–3 hours' travel time for the rain cells) from the probable source. The gradient of rainfall accumulations east–west across the rainband was as much as 30 mm in 20 km. The rainfall distribution chart (Figure 5(d)) covers the 48-hour period ending at 09 GMT on 22 October. Most of the rainfall of the marked rainband fell during the period ending about 11 GMT on 21 October and hence spanned the two 'rainfall days'.

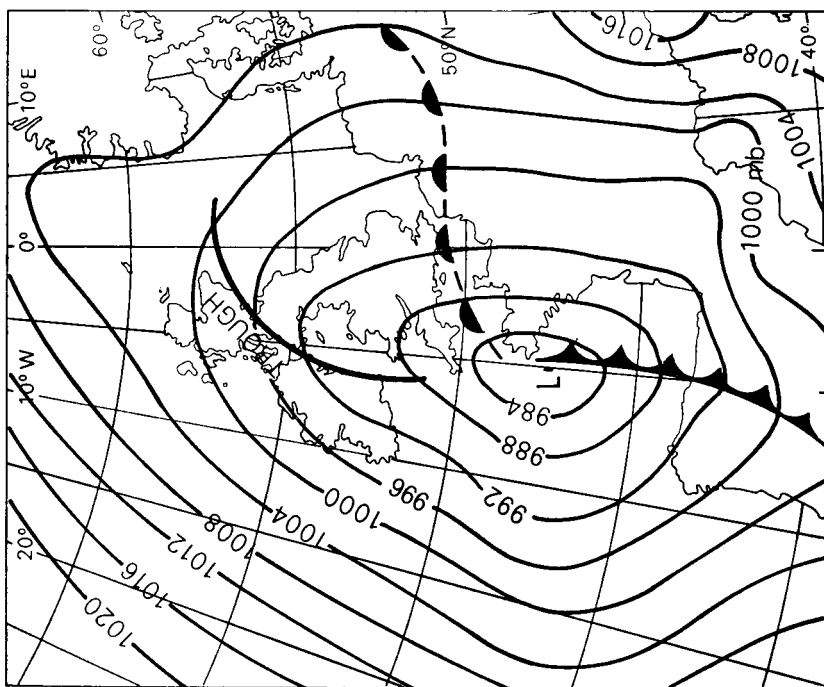


Figure 1(a). Synoptic chart for 06 GMT on 6 October 1977.

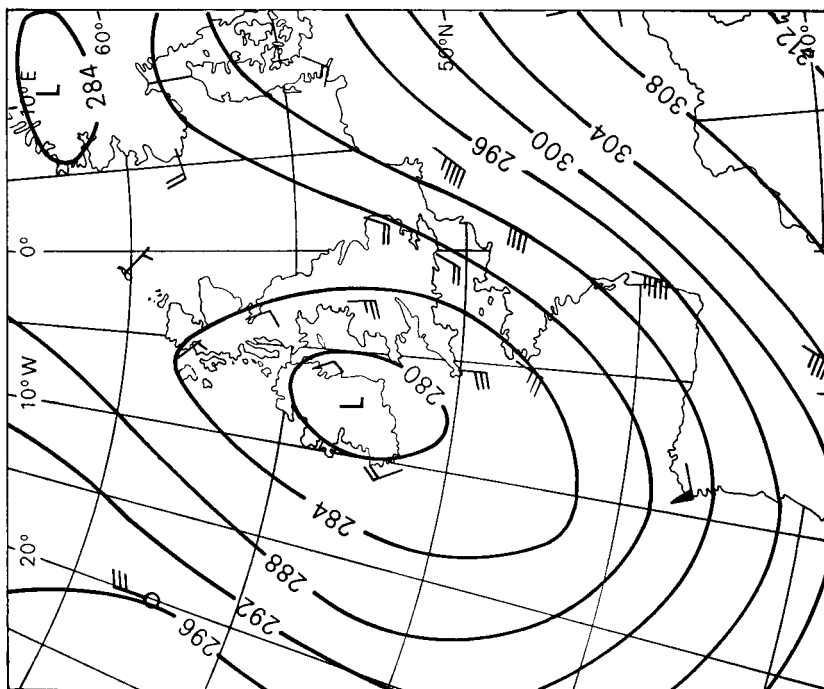


Figure 1(b). Chart for 700 mb at 00 GMT on 6 October 1977. Heights are in decageopotential metres.

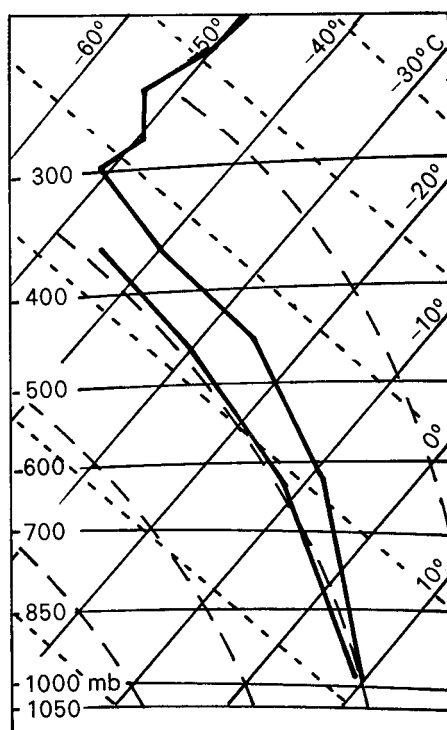


Figure 1(c). Tephigram for Camborne at 00 GMT on 6 October 1977.

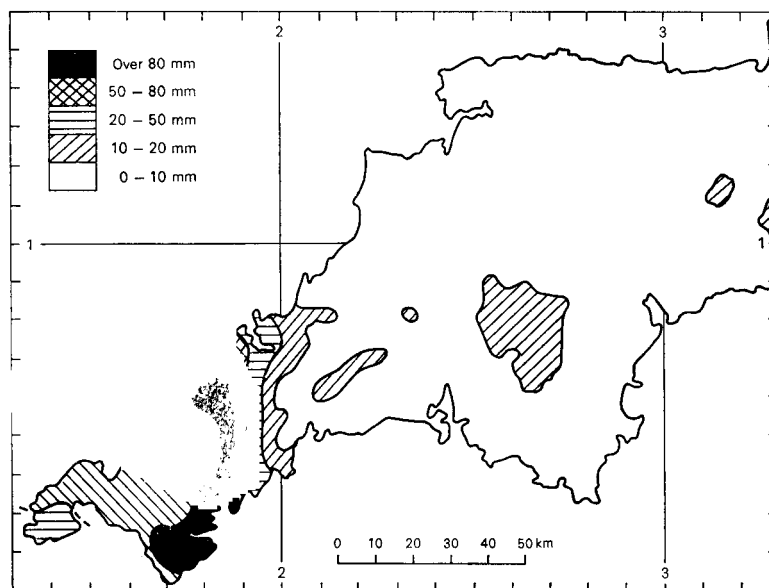


Figure 1(d). Rainfall totals for 24 hours from 09 GMT on 5 October 1977 to 09 GMT on 6 October 1977.

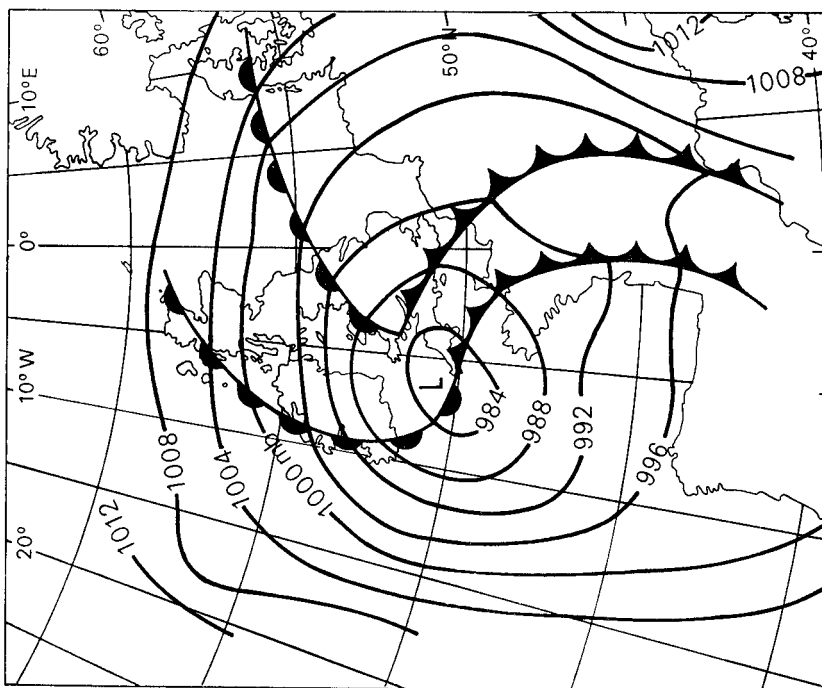


Figure 2(a). Synoptic chart for 18 GMT on 6 October 1977.

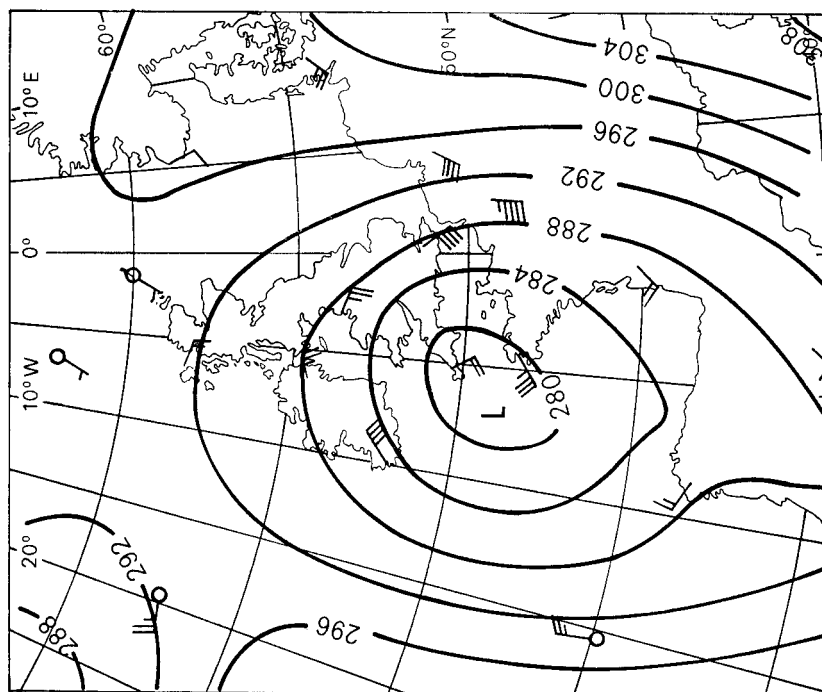


Figure 2(b). Chart for 700 mb at 00 GMT on 7 October 1977. Heights are in decapogeopotential metres.

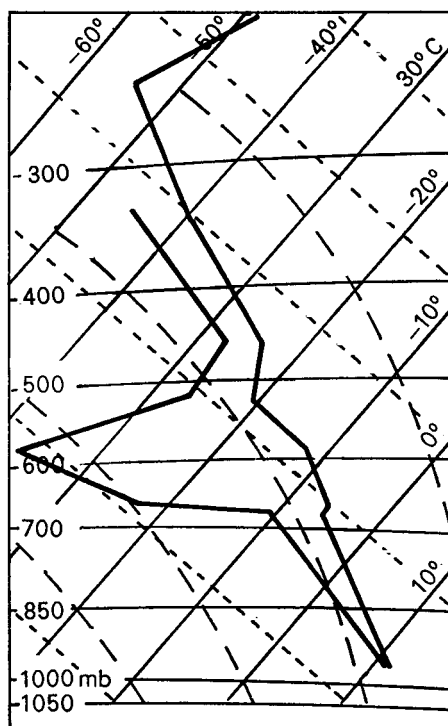


Figure 2(c). Tephigram for Camborne at 12 GMT on 6 October 1977.

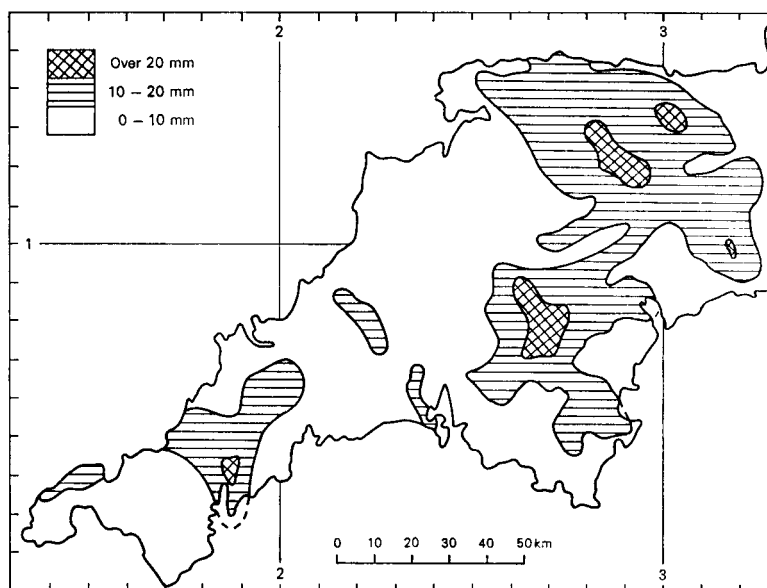


Figure 2(d). Rainfall totals for 24 hours from 09 GMT on 6 October 1977 to 09 GMT on 7 October 1977.

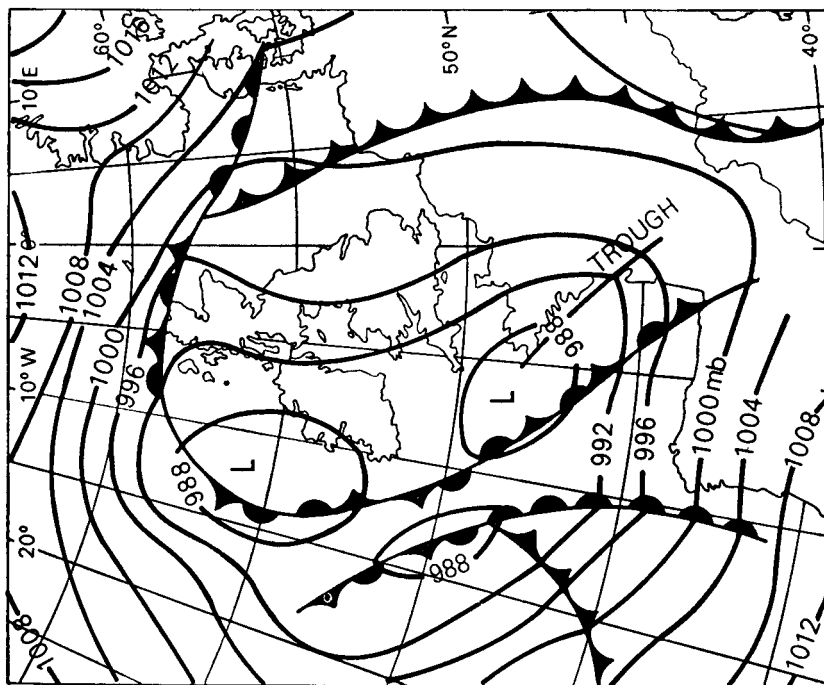


Figure 3(a). Synoptic chart for 00 GMT on 8 October 1977.

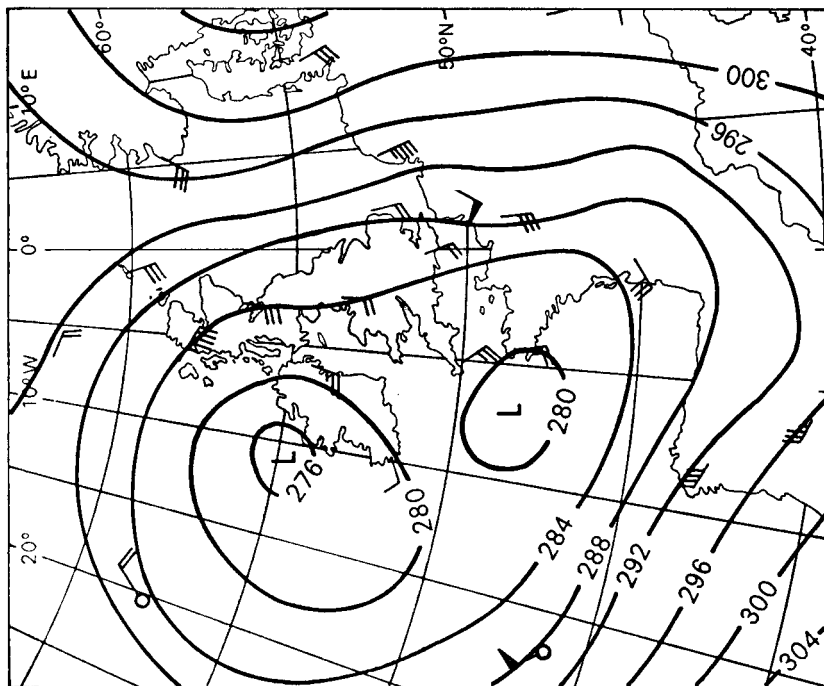


Figure 3(b). Chart for 700 mb at 00 GMT on 8 October 1977. Heights are in decapotesential metres.

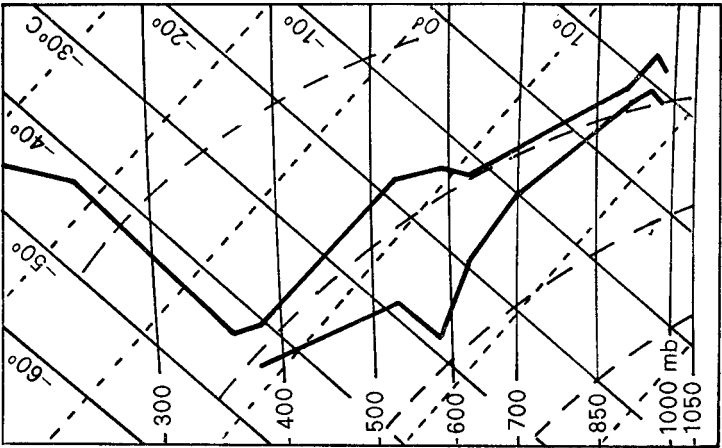


Figure 3(c). Tephigram for Camborne at 00 GMT on 8 October 1977.

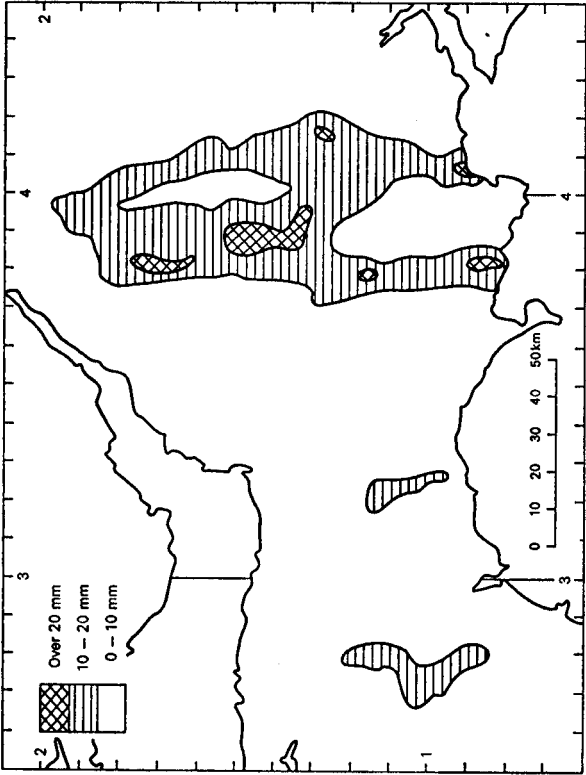


Figure 3(d). Rainfall totals for 24 hours from 09 GMT on 7 October 1977, to 09 GMT on 8 October 1977.

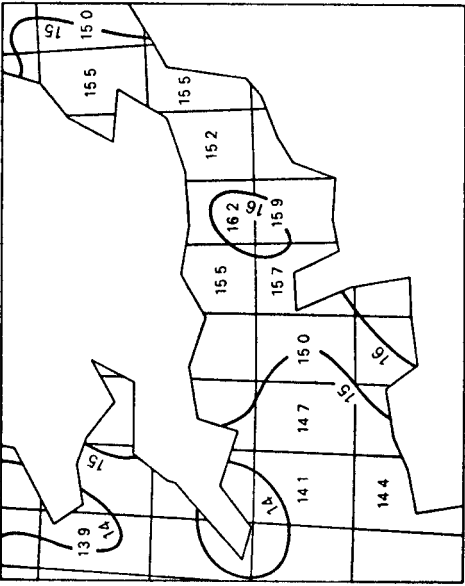


Figure 3(e). Five-day mean sea isotherms in degrees Celsius for 5-9 October 1977.



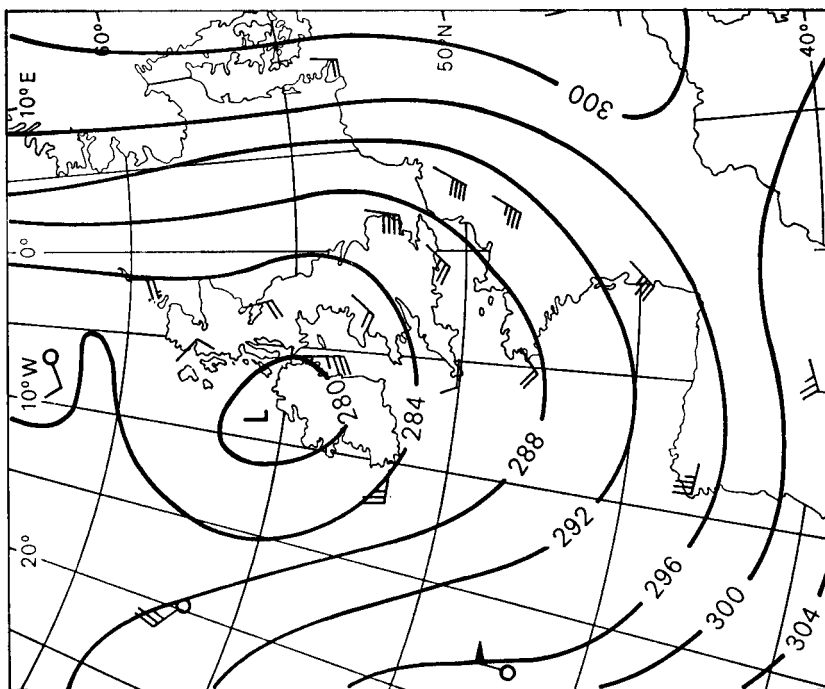


Figure 4(b). Chart for 700 mb for 00 GMT on 9 October 1977. Heights are in decageopotential metres.

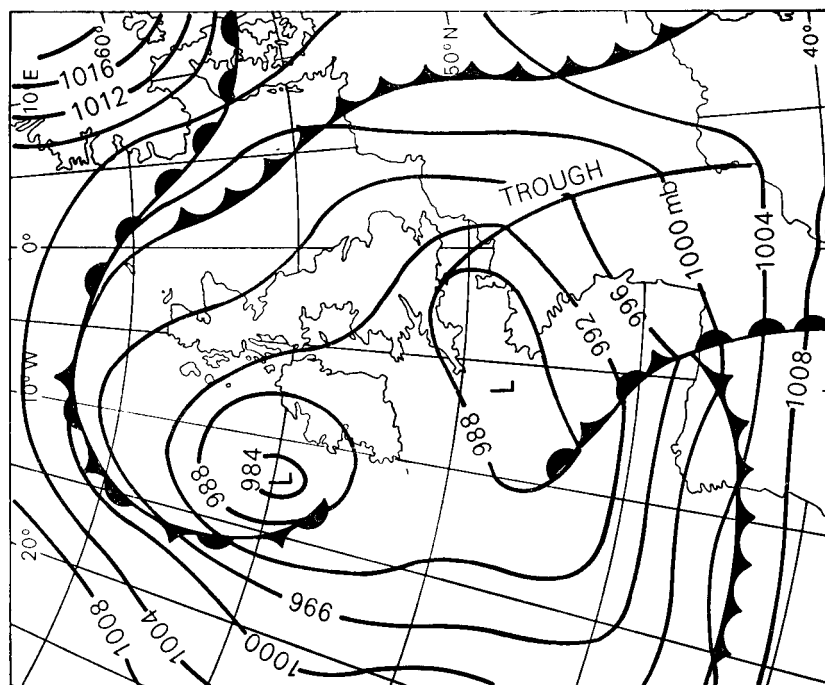
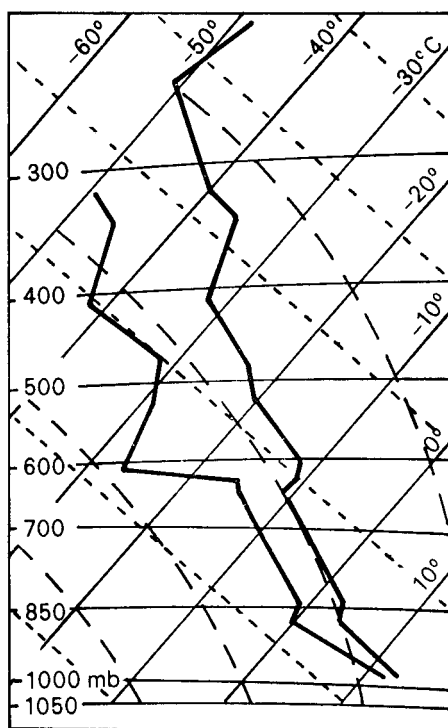


Figure 4(a). Synoptic chart for 12 GMT on 8 October 1977.



Map of the North Sea showing the distribution of the mussel *Mytilus edulis*. The map includes a legend for size classes: Over 20 mm (cross-hatched), 10 - 20 mm (horizontal lines), and 0 - 10 mm (white). A scale bar indicates distances from 0 to 50 km. The map shows high concentrations of mussels along the Norwegian coast and in the central North Sea.

**Figure 4(d). Rainfall totals for 24 hours from 09 GMT on 8 October 1977 to 09 GMT on 9 October 1977.**



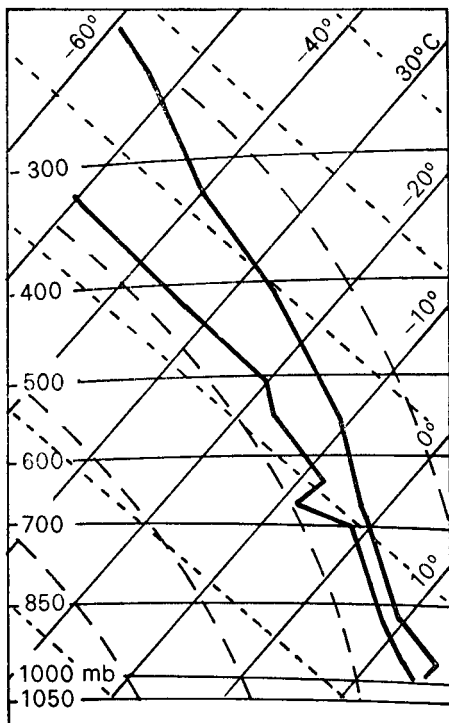


Figure 5(c). Tephigram for Crawley at 00 GMT on 21 October 1977.

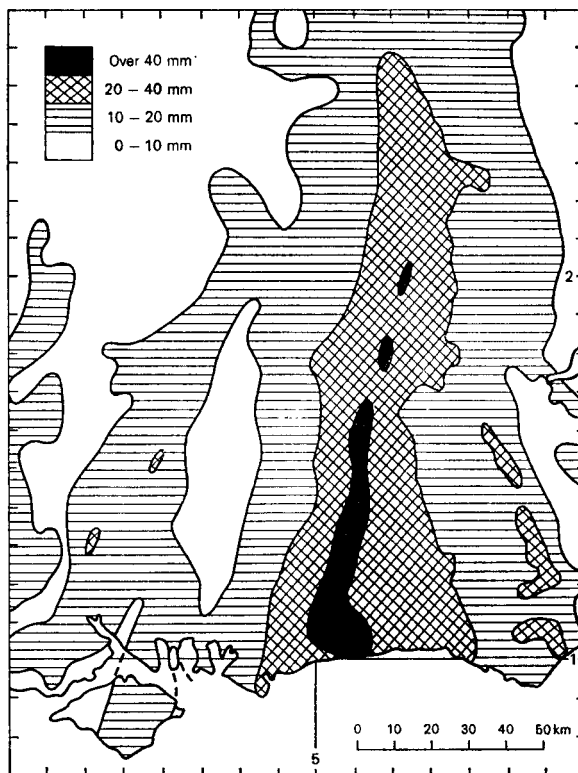


Figure 5(d). Rainfall totals for 48 hours from 09 GMT on 20 October 1977 to 09 GMT on 22 October 1977.

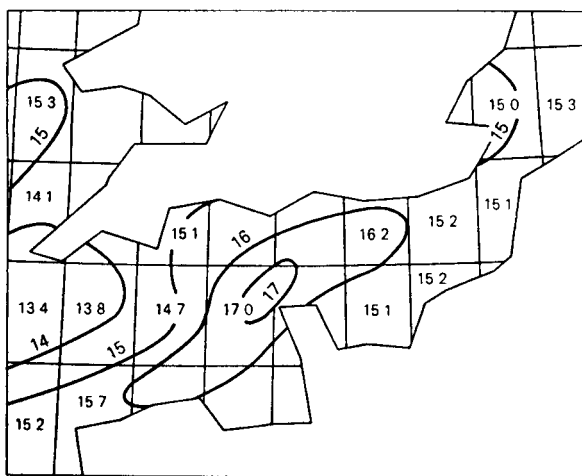


Figure 5(e). Five-day mean sea isotherms in degrees Celsius for 18-22 October 1977.

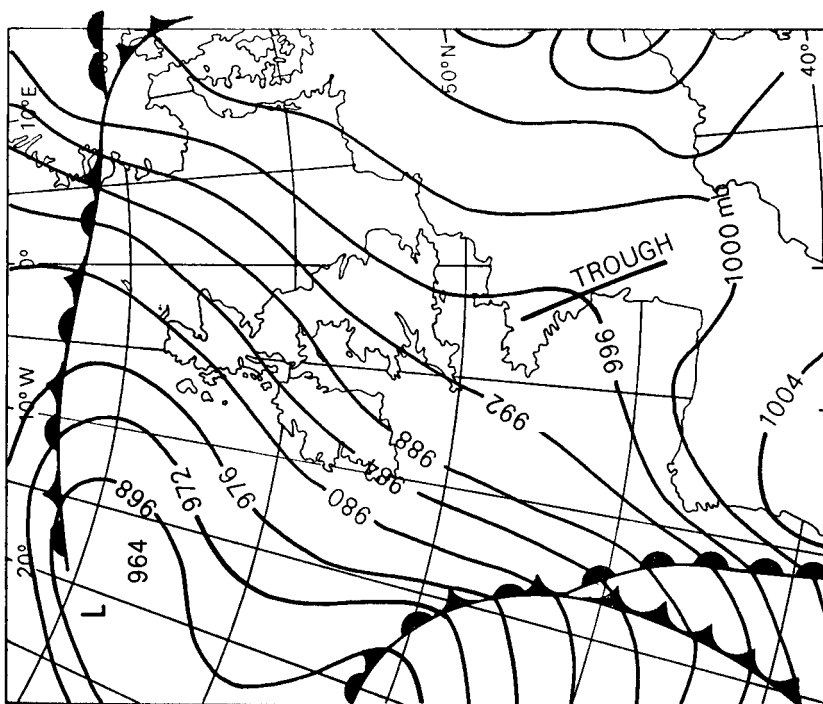


Figure 6(a). Synoptic chart for 18 GMT on 24 February 1978.

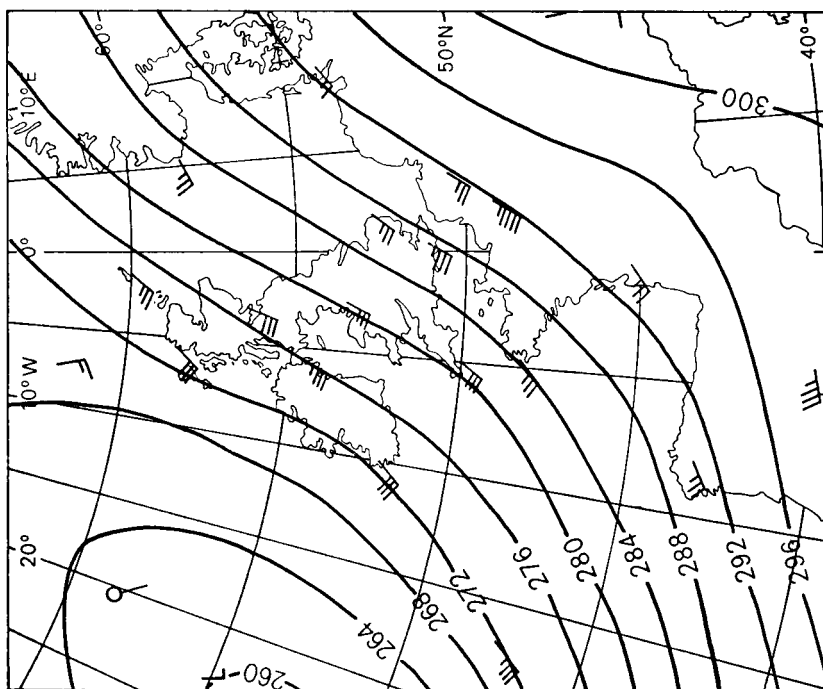


Figure 6(b). Chart for 700 mb at 00 GMT on 25 February 1978. Height are in decameters.



**Plate I.** Mr C. Kyriacou receives the B.E.M. from Major-General W. R. Taylor, Administrator of the British Sovereign Base Areas in Cyprus, at Episkopi on 3 September 1979 (see page 28).

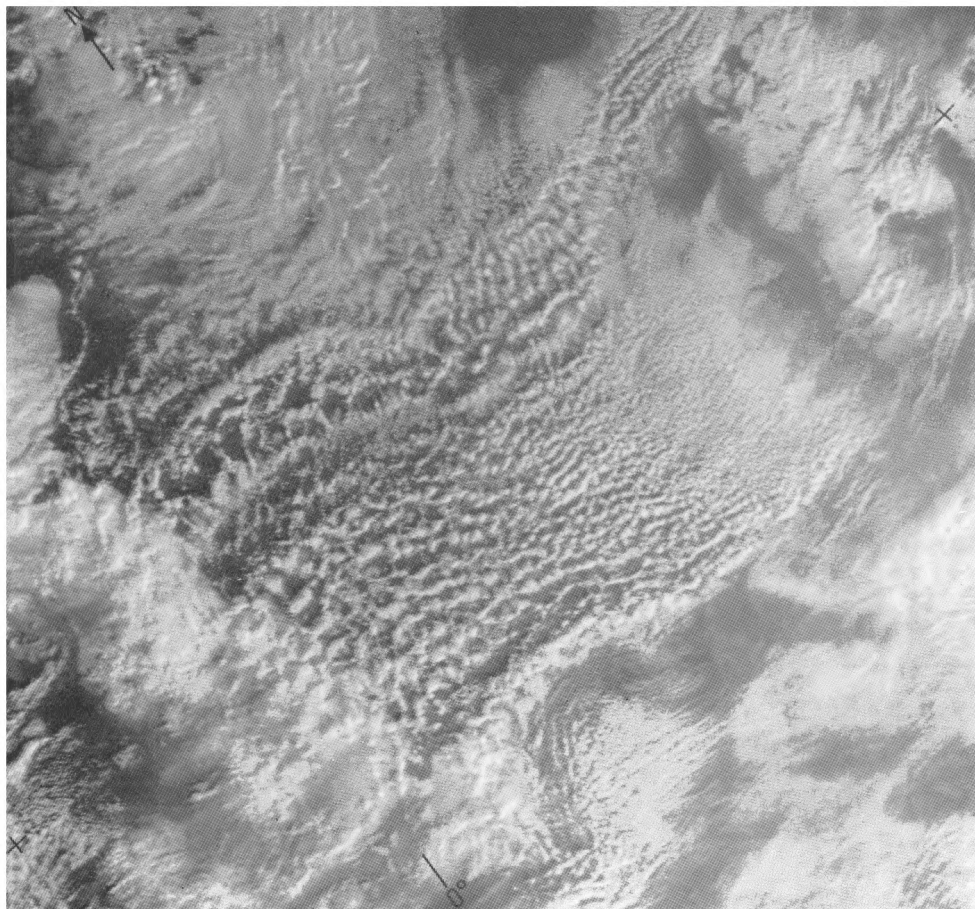


Plate II. Satellite photograph in the visible light obtained from NOAA 5 at 0952 GMT on 9 February 1978.

A very cold east to north-east airstream is flowing out over the North Sea from round an anticyclone centred over southern Norway. Note the clear areas along the coasts of Denmark and the Low Countries and how the size of the convective cells increases with increasing fetch over the relatively warm sea.



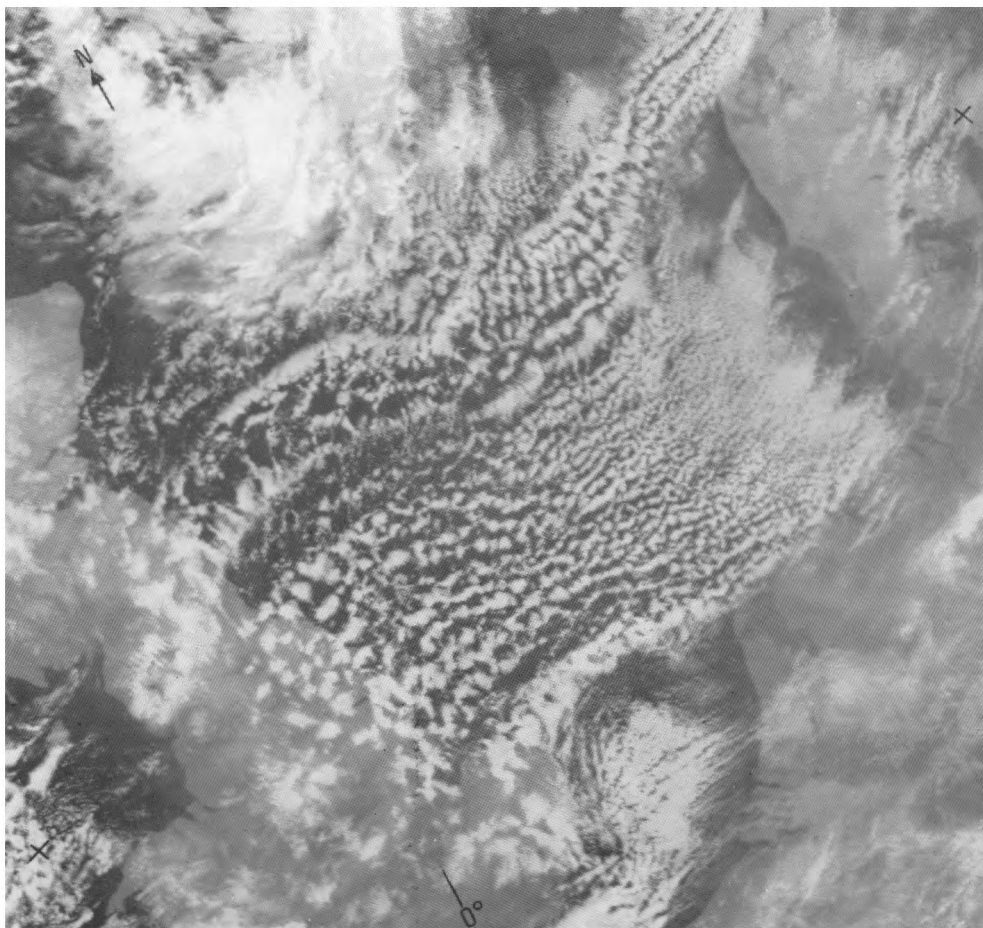


Plate III. Satellite photograph infra-red image obtained from NOAA 5 at 0952 GMT on 9 February 1978. See notes under Plate II.



Plate IV. Sketch of a ruined house at Marchépot, drawn by H. Cotton (see page 22).



Plate V. Sketch of a large barn at Briost damaged by shell-fire, drawn by H. Cotton (see page 22).

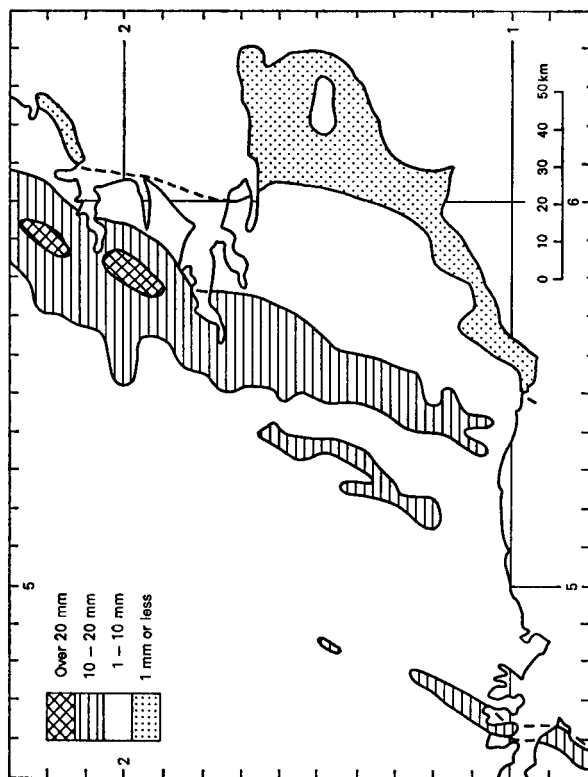


Figure 6(d). Rainfall totals for 24 hours from 09 GMT on 24 February 1978 to 09 GMT on 25 February 1978.

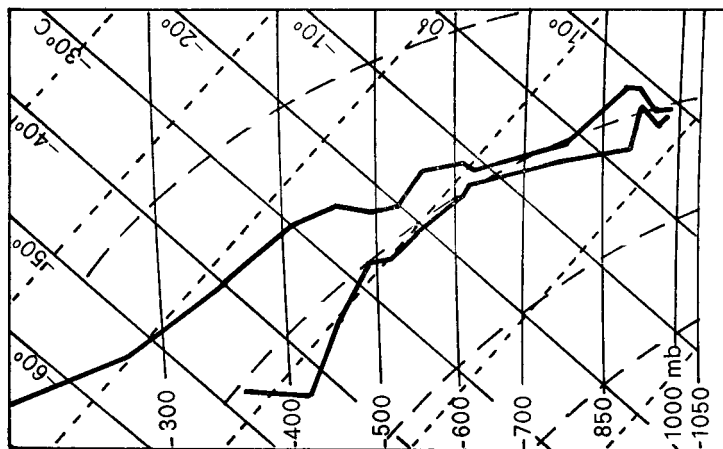


Figure 6(c). Tephigram for Crawley at 00 GMT on 25 February 1978.

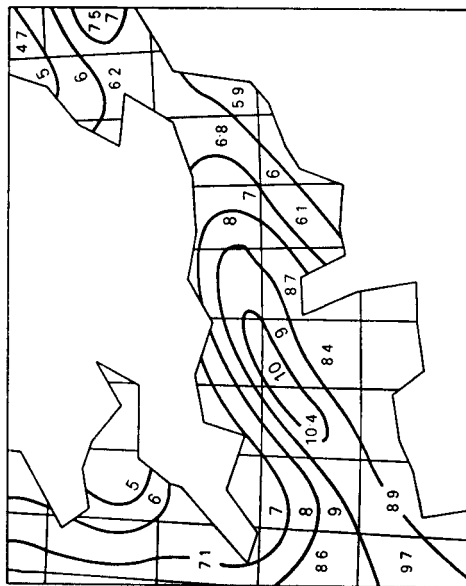


Figure 6(e). Five-day mean sea isotherms in degrees Celsius for 21-25 February 1978.

(c) 24–25 February 1978

A trough (Figure 6(a)), south-west of Brittany at 12 GMT on 24 February, moved north-eastwards across Brittany during that evening. It seems probable that a northward extension of this trough moved across the English Channel, triggering the medium-level instability shown by the Crawley 00 GMT upper-air ascent for the 25th (Figure 6(c)). A pronounced rainband, evident from Figure 6(d), extended from the South Downs, across east London and up into Suffolk, with an orientation close to the 700 mb wind flow (Figure 6(b)). The five-day mean sea isotherm chart (Figure 6(e)), covering the period, shows a marked tongue of higher sea temperatures extending along the English Channel from the south-west.

*Discussion.* By simple inspection we can infer only vertical motion; no single factor stands out as the most likely cause of the rainbands observed. Among the more obvious possible contributory factors, however, we can now see a low-level convective-heat source downwind in the Channel, a low-level convective 'heat island' source provided by the Greater London area, low-level convergence, with contrasting air from the southern North Sea being drawn into inland areas of eastern England and Kent, together with orographic uplift provided by the higher parts of Essex and the Downs to the south of London.

### Concluding remarks

The availability of computer-plotted 24-hour rainfall total maps has added a new dimension to the quality control of rainfall data. Whereas previously the rainfall at a station was compared only with that at a few neighbouring stations\*, the significance of rainbands (pre-warm-front, warm-sector, or, as discussed here, of an instability type) has now been highlighted and the coherence of patterns of rainfall from stations up to about 80 km distant is now considered before apparently unacceptably high individual station values are rejected.

Whilst a climatology of rainfall depth patterns could perhaps lead to simple objective procedures for the local area forecaster, such a development is likely to be superseded by the advent of rainfall fields observed by radar. Nevertheless, for some time to come, the forecaster is likely to have to rely on his personal assessment of some of the effects listed below in his estimates of rainfall depth and variations across a region, which the examples shown suggest may not be entirely random, even in a convective situation:

(a) *General orography*, remembering that hills only a few tens of metres high (Bergeron 1960) can affect the fine-scale distribution of daily rainfall.

(b) *Land-surface temperatures*. It has been suggested (Jackson and Wescott 1977; Atkinson 1977) that urban effects may well enhance rainfall downwind of industrial sources.

(c) *Sea temperatures*. The exact effect of heating by the sea on the distribution of rainfall is difficult to evaluate on any given occasion, mainly because of the problems of obtaining reliable and sufficiently detailed sea temperature information. However, it is likely that mesoscale variations in sea temperature are instrumental in releasing potential instability over preferred areas.

(d) *Friction*: differences in frictional effects, especially at coastal boundaries.

(e) *Low-level convergence*: 'funnelling-effect' caused by local topographic features and sea-breezes on to promontories can be contributory factors.

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\* In the Daily Quality Control computer program run by the Agriculture and Hydrometeorology Branch (Met. O. 8) normally the 6 'best-fit' neighbours out of 8 within a 25 km radius are compared with the station in question.

(f) *Surface troughs*, usually detectable on synoptic charts, often provide, by upward air motion, the necessary trigger required to release potential instability.

### Acknowledgements

I am grateful to Mr J. Stancombe and his Rainfall Data Quality Control team in Met. O. 8b for their help in analysing and drawing the maps.

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|   | 1979 | A notable heavy rainfall over west Cornwall. <i>Weather</i> , <b>34</b> , 138-140.   |

## **MOLARS—Automating the National Meteorological Library\***

By E. W. C. Harris (National Meteorological Library) and T. McSean (British Library)

### **Summary**

The functions, subject coverage and stock of the National Meteorological Library are described. About 10 000 items are added annually to the collection, which is indexed by means of the Universal Decimal Classification. Catalogue card production, updating the subject catalogues and producing the Monthly Accessions List are processed on a Digital Equipment Corporation PDP 11/34 minicomputer, the system being known as MOLARS (Meteorological Office Library Accessions and Retrieval System). Based very closely on the existing manual library procedures, it replaced Flexowriters, eliminating the necessity of sorting tapes, and requires only one entry via a visual display unit (VDU). Special features of the system include an extensive range of quality-control and error checks. The Meteorological Office's main computer currently prints the catalogue cards twice weekly (this process should be speeded up considerably with the introduction of a Diablo 1620 printer) and prepares the Monthly Accessions List of some 800 entries. The bibliography update suite is then prepared. It is proposed to introduce soon a full on-line information retrieval and enquiry service, using the Library's catalogue data base. The hardware involved is an 80 megabyte disc unit with four VDUs. The file will contain all additions to the library since 1971. A serial control system is envisaged in the longer term.

### **Background**

The National Meteorological Library is situated in Bracknell, at the headquarters of the Meteorological Office. The Library's principal function is to serve the professional needs of Meteorological Office staff, including those based at the many outstations around the country, and it has done so since its foundation in 1870. The Library also has a national role and may be used by any government department, local authority or serious enquirer. These 'outside' queries cover a surprisingly wide range of activity, including military matters, civil aviation, agriculture and civil engineering (with particular reference to overseas contracts).

The Library aims to collect most available meteorological and climatological published data and literature from all over the world. To a more limited extent it also collects material in related disciplines such as fluid dynamics, hydrology, oceanography, and planetary atmospheres. General material on other disciplines (e.g. physics, computer technology and mathematics) is also made available. The 120 000 volumes in stock consist predominantly of data, reports and journals. Conventional monographs represent a comparatively small proportion of this figure—currently only 100–150 textbooks are acquired each year. In addition, the Library maintains a pamphlet and offprint collection of over 30 000 items and there is a very large collection of daily weather reports from weather stations throughout the world, with some records being continuous for over a century. The collection mainly consists of printed material but microfilm and microfiche are becoming increasingly common. Much of the Library's material is acquired under international exchange agreements, of which there are currently more than a thousand.

No direct use is made of any public on-line information retrieval service, mainly because the Library operates mostly within a very narrow subject range and within that range the main data base currently available would not usefully improve either the depth or the currency of the service offered. Neighbouring government departmental libraries with facilities for on-line searches help out with the occasional query requiring a broader subject approach.

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\* Adapted from an article in *VINE* 28 (Southampton University Library, 1979).

## The catalogue

Library staff, who have a background of meteorology rather than librarianship, are always on hand to advise and assist users in getting the best out of the collection, but self-help is encouraged as much as possible. An author catalogue, including both personal and corporate authors, is available on some 400 000 cards. Subject indexing is detailed, and the subject catalogue is a loose-leaf compilation (in effect a series of subject bibliographies) housed in nearly 200 large binders. About 10 000 items are added to the catalogue each year, including individual journal articles and conference papers. The author catalogue dates back to the end of the nineteenth century and includes the occasional hand-written card. All additions to the catalogue since 1936 are indexed by means of the Universal Decimal Classification (UDC) which includes highly detailed schedules for climatology and meteorology. Each item is assigned an average of 2.5 class numbers.

The cataloguing code in use is that given in the World Meteorological Organization's *Guide to Meteorological Library Practice* (WMO Publication No. 39, TP14, 1955) which varies from the codes of the Anglo-American Cataloguing Rules—for example the sponsoring organization is always preferred to the individual author for the main heading when both are present. In addition, there are 'special' bibliographies for climatology, divided geographically, and for papers by members of the staff.

One of the most useful tools produced by the Library is the Monthly Accessions List, which is set out in classified order. More than 250 copies are printed for distribution within the Meteorological Office and to outside organizations (80 of which are overseas) and the list has proved an invaluable link between the Library and its sometimes rather dispersed user population.

## On-line system

From the beginning of 1978 the first stage of a program of computerization has been in operation within the Library, with catalogue card production, the updating of the subject and special catalogues, and the preparation of the Monthly Accessions List all carried out by the use of a Digital Equipment Corporation PDP 11/34 minicomputer dedicated to Library use. The system is known as MOLARS (Meteorological Office Library Accessions and Retrieval System).

The Meteorological Office was a very early user of computers. At one time it boasted the most powerful machine in the country, and even today its massive IBM installation, comprising a 370/158 plus a 360/195, is among the five biggest. When the Library began to make plans for computerization it was fortunate to be able to call upon the considerable expertise of the Office's Systems Development Branch for systems analysis and programming. All the necessary software has been developed by the Branch, which also gave advice on the type and range of hardware required for the system. While it has been extremely useful to have the programming team so close at hand, the Library's comparatively low operational priority means that innovations may take some time to be translated into working systems.

The new system has been based very closely on existing proven manual library procedures. In some ways the whole process of analysis was made easier because of the specialized nature of the Library, and until the second stage of the computerization comes into effect—with on-line enquiry access to the catalogue—the visible operational procedures of the Library will not have been greatly affected, except that things now tend to happen rather more quickly than before.

The new system replaces one using Flexowriters that was introduced in the early 1960s. These machines eliminated the need to retype each entry completely in each catalogue sequence; however, the paper tapes had to be reread or reformed (or both) at least four times for each entry and a great deal of sorting of tapes to satisfy the various formats was necessary. Under the new system only one entry via a visual display unit (VDU) is required—all changes of format are done by the software, not by the



typists. For each item received by the Library an input form is prepared. (As already mentioned, every item of interest in conference proceedings and new issues of journals is normally catalogued and classified separately.) The format of the input forms is matched by the format provided on the VDU, with the discrete ('protected') fields shown for ease of input. Most of these fields are obvious: author, title, source, miscellaneous (e.g. notes), UDC number, shelf location and catalogue. When multiple authors are cited in the author field, cards for each author are produced. When the main access point to a record is a corporate author, the system is able to spot the name of one or more personal authors in the title field (preceded by the word 'By') and to generate automatically the necessary added entries for the catalogue. Classification is very detailed, and where more than one class number is assigned the most important number is indicated by an asterisk because each entry only appears under one classification in the Monthly Accessions List.

A number of special features have been built into the input and output ends of the system. Particularly worthy of interest is the range of quality controls and error checks that has been built into the input routines. Checks are made on the punctuation, layout, number of characters, and so on in each field and, where appropriate, the presence or absence of alphabetic and numeric data is also checked. Likely errors are printed out to be checked by a member of staff. Many of these 'errors' turn out to be unusual rather than wrong, but nevertheless these routines are extremely useful as a supplement to normal proof-reading. The range and scope of automatic validation of input within the system undoubtedly stems from the high priority traditionally given to such things in weather forecasting programs, with the Meteorological Office Library benefiting from the Systems Development Branch's experience. Other aids to input have been incorporated. An on-line data base has been built up containing standard 'Source' details (title, imprint, etc.) for periodicals catalogued in depth by the Library. Correct details are added to a catalogue entry simply by keying in the appropriate Source index number. Pagination can be added to the short references and transient information (volume number, year, etc.) can be updated easily by the operator. Codes built into the catalogue format allow variations to the standard output: ST, for example, indicates an item written by a member of staff and generates an entry in the staff bibliography.

The Meteorological Office's massive main computer complex (COSMOS) is used for major sorting of files and (for the time being) for the twice-weekly printing of catalogue cards. Special continuous card stationery is used, giving four cards per page, which is later guillotined to the Library's historical 'standard' size. The loading of the specialized stationery and the setting up of the special print train required to print lower-case type has proved to be uneconomic for the necessarily short catalogue print runs. This problem should soon be overcome because the Library has acquired a Diablo 1620 printer. In the near future this will be used for card production, making possible high-quality printing on ready-cut card stock and the twice-daily printing of batches of cards, cutting out the present hiatus between the item going on the shelf and the card appearing in the catalogue. The printer is also being used for error messages and other systems output.

In the normal course of events, all the catalogue data for a particular month have been input by the second working day of the following month. The production of the Monthly Accessions List can then be initiated. This is also carried out on the COSMOS machine from the inverted files which have been built up from the card production runs. The program sorts the entries, inserts item numbers and pagination, and a contents page is prepared. The List is then output on to 35 mm film which is proof-read. If there are major errors these are corrected and the program is rerun; otherwise a full-sized camera-ready copy is prepared and dispatched to the printers. MOLARS has proved to be about three weeks quicker than its manual predecessor in producing the Monthly Accessions List, which is now available for distribution before the end of the following month.

A typical List contains some 800 entries, from which about 1250 author cards will be produced and some 1800 entries for the classified catalogue.

Once the List has been cleared for publication, COSMOS runs the program for the monthly updating of the subject bibliographies. Like most of the other Library work on COSMOS, this work is carried out in low-priority time, mostly at weekends. The Bibliography Update suite produces all the pages required to update existing bibliographies, reprinting all those pages that were not full and for which there are new entries. The bibliographies are kept in accession order, so a complete reprint for each new entry is not necessary.

### **Further development of MOLARS**

There is no doubt that only a small part of the potential of the minicomputer has so far been realized. However, the first stage of the system has produced a useful range of services and has introduced the library staff gradually to the advantages and perils of automation. The noisy (and increasingly temperamental) Flexowriters have been replaced by more congenial VDUs; typing time has been reduced to a third of its previous level.

The next stage of automation will be the introduction of a full on-line information retrieval and enquiry service using the Library's catalogue data base. Within the present system a rudimentary information retrieval system is already available: entries from the present and preceding month may be recalled by keying in the first eight characters of the author's name. What is envisaged is a much more comprehensive system using the entire catalogue file with access via any part (or combination of parts) of the catalogue record and available to the Library's users as well as its staff. The idea will be converted into software as soon as the Systems Development Branch has the staff time available. The hardware, consisting of an 80 megabyte disc unit plus four extra VDUs, is already available. The additional video-terminals are less elaborate and costly than the two Lynwood VDUs currently used for inputting and editing catalogue data. A disc file has been created using the paper tape produced under the Flexowriter regime, which contains all additions to the Library catalogue since 1971. When added to the MOLARS file proper, it will give a data base of up to 100 000 records. When made available to on-line queries this should be adequate for 70-80 per cent of all searches. When this part of MOLARS goes live the Library will cease adding new cards to the catalogue and this in itself will be a considerable boon, since in recent years the catalogue has been growing in size alarmingly and taking up space that could be used more usefully for other purposes. If time permits, the Library may weed out cards for items within the on-line system, thus reducing the card catalogue to more manageable proportions.

In the longer term, the Library is considering a serials control system, which would also be able to monitor the more than 1000 exchange arrangements in which the Library participates. A statistical and management subsystem may also eventually be grafted on to MOLARS, but the Library intends to continue its steady one-step-at-a-time approach which gives the best guarantee of maintaining a consistent level of service to its customers.

## Memoirs of an Army Meteorologist

By H. Cotton, M.B.E., D.Sc.

### Part 4

(Continued from the November 1979 issue)

'What do you intend to do this evening? It is such a beautiful evening that it is a shame to waste it.' This was Corporal George speaking. I had intended to swot some new German after the 6 p.m. balloon ascent was finished. To a scientist and an engineer German is a very important language and I was not all that good at it.

'Have you something in mind?' I asked.

'Yes, I thought we might go sketching. There must be some interesting material in these towns', and he pointed to Marchélepot and Briost on the map.

'There are at least two hours of daylight left, so I shall have time to make one or two watercolour sketches. There are some ruined houses at Marchélepot which will just suit you with your liking for fine detail, although I wish you wouldn't niggle so much.'

I found a convenient seat on a fallen tree and made a sketch of a ruined house at Marchélepot. I then went to find Corporal George who was making a very attractive painting of a very large barn at Briost, a village on a slope just above the river. The barn was severely damaged by shell-fire and all the more interesting because of it. As the painting was not quite finished I decided to make a sketch of my own. I wondered what he would say—I felt rather proud of my two sketches.

'Your drawing of Marchélepot is quite good, although you still don't let yourself go. The same with the barn. I think you ought to try charcoal for a change. You won't be able to get in so much detail, but your drawings will have more character.'

He pointed to two lines I had drawn across the sky on my sketch of the barn.

'Whatever are these?'

'Cloud of course.'

'Oh, what kind? There isn't any cloud anyway.'

'It's supposed to be a band of cirrostratus.'

'I've never seen cirrostratus looking like that.'

'Well I have.'

'But there isn't any cloud. Put in what you see, not what you don't see and then your drawings will be more convincing and more honest. You can leave things out for the purpose of composition and if they will be a distraction from the main subject, but that is not the same thing as putting in things that aren't there.'

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Early in July 1917 we learned that 4th Army H.Q. was to move to Bray Dunes on the Channel Coast. I was sorry in a way to leave the quiet of Villers Carbonnel, but a spell 'by the seaside' was exciting. The pleasure was tempered by the certainty that such a move, from the very south of the Army to the extreme north, could only mean one thing. There was to be another offensive before winter set in. By now the vital importance to the Army Commanders of reliable meteorological information covering all

aspects of operations was fully realized: one no longer received the jibe about one's cushy job. With the abandonment of that unreliable weapon chlorine gas supplied from gas cylinders, the necessity for the early gas alerts and warnings no longer applied. But more and more the Commanders relied on accurate weather forecasts—and considering the then scarcity of information in comparison with today's plethora, the forecasts were remarkably accurate. Corrections for the artillery were also of progressively increasing importance, as I was soon to find out.

The lorry containing all our equipment belonged to the Army Service Corps and so did the driver. I travelled with him in the cab but the others went by car so as to be ready for us as soon as we arrived. We took two days over the journey, and very pleasant they were, as the route was through undevastated countryside. We stayed for the night at the little town of Pernes, at a hotel in one corner of the Grand' Place. I sensed that there was something wrong with the place and realized what it was as I was sitting at a little table on the pavement in front of the hotel; the driver had gone off on his own somewhere and I had a feeling that he had been here before. The trouble with the town was its unexpected quietness, there being few people about. Then it dawned on me: no men, and no-one came for a drink; in fact, I believe that the two of us were the only guests in the hotel. As soon as I finished my drink I decided to go for a walk. I should have enjoyed it, as it was pleasant enough, but I could not get rid of the sadness of the place.

As I had a separate bedroom I had no idea whether the driver stayed the night at the hotel. I had a feeling that he did not, but he was there for breakfast. Our schedule was to arrive at the Kursaal at Bray Dunes not later than 5.30 p.m. so as to give time to set up the theodolite and be in good time for the 6 p.m. balloon ascent, an important ascent because of the delay occasioned by the move, because the Belgians who had previously manned the sector adjacent to the sea had not made upper-air observations, and because of the effect of land- and sea-breezes which sometimes resulted in a surface wind being almost diametrically opposite in direction to the wind in the upper air.

The driver took the longest route, which was not scheduled and could have been avoided, although the alternative route was not so good. He drove the lorry to a large shed somewhere in the middle of Dunkirk, jumped down, said 'See you in the morning' and began to walk away.

'Where do you think you are going?' I said, 'this isn't a pleasure trip; we have to be at Bray Dunes by five-thirty.'

'Well we won't will we.'

'Why not?'

'Because I've got a girl in Dunkirk.'

'Look here' I said, 'you can't do that. I order you to take me to Bray Dunes now!'

'Who the bloody hell do you think you are? I take my orders from my officer and from nobody else' and he went off to his girl; I couldn't prevent him from doing that. But my presence of mind deserted me and I accepted the position and did nothing. Bray Dunes was only seven or eight miles away and I could easily have walked there, informed Meteor of the situation and possibly have arranged for another A.S.C. driver to bring in the lorry. I tried to find an M.D., although by then it wouldn't have been of much use, but I couldn't find one and concluded that they must have had girl friends in Dunkirk as well.

The driver turned up next morning at about half past eight, so I had missed not only the 6 p.m. observations of the previous day but the 1 a.m. and 7 a.m. as well. There would be hell to pay, and there was. The artillery in the coast sector had been supplied with wind corrections by the Second Army, further south and where there was no modification due to the proximity of the sea. For the shorter ranges the corrections were entirely wrong and it was our own men who had been shelled. I explained the situation and then set up the theodolite just where we were, opposite the Kursaal, and the telegrams

were sent first priority. What happened to the driver I didn't enquire, as I was kept busy helping to prepare the station for normal working.

To anyone who has made measurements of the surface winds only, in areas where the wind is influenced by the sea, but not of the winds at higher altitudes, the results of such observations can be quite surprising. There can be occasions when the surface wind is almost diametrically opposite to the wind at high altitudes, the gradient wind, and it is then necessary to change the orientation of the theodolite telescope quite rapidly in order to keep the balloon in the field of view. Since the observer has to take his eye from the eyepiece in order to read the verniers and call the readings to the recorder there is always the possibility at such times that the balloon might have moved out of the field of view, finding it again sometimes being very difficult and resulting in a loss of time, a serious matter because of the limited time interval between successive readings. From the observational point of view it is therefore fortunate that the sea- and land-breezes do not extend to a very great height, sometimes to no more than a few hundred feet; the finding of the balloon, if momentarily lost, is therefore facilitated by its nearness and consequently comparatively large size in the field of view. Above the sea winds are the gradient winds, as given by the direction and spacing of the isobars. With large differences in the directions of the winds at the surface and at high altitudes it is not uncommon for the corrected winds for small calibre guns to be almost opposite to those for higher calibres. It was this which caused the shelling of our own areas when corrected winds for an area not influenced by the sea were used by the artillery near the coast.

The observational difficulty caused by rapid changes in orientation does not arise at places away from the coast. There, the wind is normally a gradient wind at all heights, and the changes in orientation are so small that the balloon is lost not through wandering out of the field of view but through entering cloud or becoming visually so tiny that it can no longer be seen.

The promenade at Bray Dunes ends at the eastern end at a very high sand dune and east of that, close to the sea, is a line of lower dunes, behind which the land is flat. There are no sand dunes the whole length of the promenade, but firm sands, not quite covered at high tide. Built into the dunes east of the high isolated dune was a small fort manned by French soldiers. I never saw it in action except on one night when they shot down one of our pilot balloons by machine-gun fire. The light, steadily rising into the sky, was clearly something new to them, and they had to be informed the next morning. Our hut was placed on the flat land at the foot of the high dune, and on the top of this was a wooden platform for the theodolite.

The weather was fine and warm on the whole, and in between observations we could enjoy sea bathing. I learned for the first time the danger of the undertow. The waves of the sea are circularly polarized, that is to say the water particles, instead of moving up and down, move in circles, the direction of the wave-front being at right angles to the plane of these circles. At the top of the crest the water motion is forward, but in the long trough the direction is out to sea. If the waves are high enough the velocity of the undertow can be surprisingly high, and it is augmented when the tide is ebbing (because of the gravitational pull of the moon.) One afternoon at a time of ebb tide I was bathing by myself and unwisely went beyond the breakers. On the shore side of the breakers there is no danger because friction reduces the undertow, but beyond these there is no such effect. The current was so strong that I realized I could not get back. Fortunately there were rows of groynes running out to sea; I managed to work my way to one of them and literally pulled myself hand over hand to the shore. A more amusing incident was when Banaghan and I had just finished bathing and were walking across the beach, stark naked, when General Rawlinson rode by. Banaghan stood stiffly to attention and saluted. I did nothing, remembering that one should not salute if not wearing a cap. As far as I know the rule said nothing about the rest of one's clothes.

The Meteor office was a good-sized room on the first floor of the Kursaal, no doubt previously used as a bedroom. A few yards away at the side of the road was the Post Office, housed in a large Nissen hut, and opposite to it a large car park for staff cars. One day I was just about to leave after enquiring about mail when there was a series of loud explosions, very close. Like everyone else I threw myself flat on the floor and hoped for the best. Everybody knew that there was an offensive in the offing and it looked as though the agreement regarding the shelling of Army and Divisional Headquarters no longer existed. The bombs, for that is what they were, were obviously intended for the Fourth Army H.Q. but, luckily, they only killed one man and a cage-full of carrier pigeons. The man was given a full military funeral, and, never having seen one before, I felt shocked when, on the return, the band played a jolly marching tune.

Besides the R.F.C. there was a squadron of the R.N.A.S. operating in the area. I had occasionally seen a young man in an immaculate blue uniform lolling in the back seat of a Lancia saloon, and two ratings in the front, one the driver and the other no doubt to open doors. I was surprised when he turned up one day just as we were about to make the 1 p.m. balloon ascent. By that time we were rather scruffy, with worn, not very smart uniforms, and somehow I resented the arrival of this beautiful apparition. He explained that he had noticed our balloons from time to time and guessed what we were doing. Would we mind if he watched, he asked, and of course we said no. I did the recording and manipulation of the slide rule and I could sense his astonishment when he realized that I was working out the results in the one-minute intervals between the readings. It was a good ascent, to 15 000 feet as far as I can remember, and it was only a matter of a few minutes before I had completed the computations, made out the telegrams—I knew the codes by heart—and sent Banaghan off to Signals. I made a copy of the data of wind direction and velocity versus height and gave it to him. He was quite unnecessarily grateful and I learned the reason why. As he left with his driver the other rating lagged behind and whispered that he only made one ascent per day, in the morning, and that it took him all day to work it out. I only hoped that the R.N.A.S. pilots didn't mind if they encountered a wind entirely different from that predicted for them. The world was an unjust place, I mused. Here was I, university man with a first-class honours degree, a mere Corporal dressed in a scruffy uniform, and there was he, obviously incompetent for the job somebody had wangled for him, dressed in a beautiful blue uniform and driven about in a dream of a car.

A pilot lives with the weather; from his point of view it can be his friend or it can be his enemy and it is the function of his meteorological advisers to tell him which to expect. This applies even today, but far more so in the days of the First World War, with gimcrack planes, to fly which at all was a miracle. 'Jobs for the boys' is, unfortunately, an inevitable characteristic of business and politics, but it is wholly reprehensible when the fate of a nation and the freedom of its people is at stake. I could never understand why, with competent Meteor observers on the spot, the R.N.A.S. did not ask for the complete information which only Meteor could supply, instead of relying on a man who, however admirable as a person, was obviously not competent to hold such a responsible position.

I found a bookshop in Rosendael, a walk of about four miles, and bought a copy of Alphonse Daudet's delightful *Lettres de mon moulin*. It was a pleasant change from the German texts I had been reading. He had a name of his own for all the bright stars. '*Voici le Râteau ou les Trois rois* (Orion). *Un peu plus bas brille Jean de Milan, le flambeau des astres* (Sirius). *Sur cette étoile là, voici ce que les bergers racontent. Il paraît qu'une nuit Jean de Milan avec les Trois rois et la Poussinière* (the Pleiades) *furent invités à la noce d'une étoile de leurs amies.*' These delightful imaginings seemed to be centuries away from the times in which I was then living. A beautiful land of make-believe which can exist only in a happy mind.

Apart from sea-bathing, an occasional cinema show, or a visit to Dunkirk, the work at Meteor Fourth

Army was exactly the same as at the other Army Headquarters. A change from routine was afforded by a request to assist at artillery ranging tests at Coxyde, some miles east of Bray Dunes. I was detailed to do this. A pilot-balloon ascent was made immediately before I started, so that the only equipment I had to take with me was the portable anemometer and a stop-watch. I went by coast road, called by the Army the Aeolian Road, on the motor bike and found the artillery testing station near the coast, with the guns, of various calibres, all pointing out to sea. I explained the pilot-balloon results to the officer in charge and worked out the corrected winds for the times of flight to be used during the tests. I also determined the surface wind just prior to each firing, it being relevant to short ranges, but of no importance at long ranges for which the shell spent a negligible amount of time in the lowest levels. It was interesting to stand directly behind the gun and watch the shell, like a black ball with a fuzzy edge, no doubt due to the rotation.

The Germans had perfect observation along the Aeolian Road because of the flat terrain and their artillery occasionally indulged in a strafe, which may have been a scheduled operation, or even pot shots at anything observed. Actually the road was used very little and, as far as I remember, I did not meet a single vehicle or party of men all the way to Coxyde. On the way back they started shelling the road about a mile ahead of me, and I flattered myself that, having seen me, they put on a show for my benefit. All I could do was to sit by the roadside, smoke a cigarette or two, and wait until they decided to stop.

During the First World War there was published a journal called *Blighty* which was circulated to the troops in France. Obviously it was by no means highbrow, but consisted of light reading and illustrations, all pen-and-ink drawings. In one issue there was a competition open only to members of the Armed Forces. As I still had my monster revolver I regarded myself as eligible. They were offering two prizes, one of one pound, the other of ten shillings, for the two best drawings which would be published in *Blighty*. I decided to have a go. At the entrance to the farm at Le Touret there were, on either side, two trees whose overhanging branches would have produced a conventional heart-shaped space in between if the bases of the trunks had been much closer together. I therefore made a drawing of the trees, modified in this way, and in the space between drew a silhouette of a soldier saying 'good-bye' to his girl. I did not suggest a title, but it was published with the caption 'Hearts are Trumps'. I obtained the second prize of ten shillings, a useful addition to a Corporal's pay, and a few weeks later I received an upside-down pipe. That, I think, is the best description. It looked like an ordinary pipe, but the opening of the bowl was at the bottom, the top being closed. There must have been a duct leading from the top of the bowl, through the briar to the stem. As I didn't smoke a pipe I gave it away. It must have been a pig to clean. I also received an offer of marriage from a girl in Wisbech.

A little while after the war I was in Liverpool Lime Street Station and as I had some time to spare I looked at the books and things on the bookstall. I was surprised to find my drawing of 'Hearts are Trumps' in the form of a picture postcard, and this time coloured, blue and pink as far as I remember.

(To be continued.)

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## Review

*Climatic change, agriculture and settlement*, by M. L. Parry (in the series *Studies in Historical Geography*). 225 mm × 135 mm, pp. 214, *illus.* Dawson Publishing, Folkestone; Archon Books, Hamden, Connecticut, 1978. Price £9.00.

This book provides what is on the whole a useful conspectus of the influence of climatic change on the cultivation of certain crops and hence on the productivity of farming and the consequential changes in population levels and patterns of settlement. Attention is focused chiefly on north-west Europe, but examples are drawn from all over the world including ancient Mediterranean civilization and the American Great Plains in modern and prehistoric times. It is not free from faults, however, and one of the most serious and fundamental is the lack at the outset of any adequate discussion of the meaning of 'climate' and hence of 'climatic change'. Under 'Climate, definition of' the index directs us to p. 25 where all that we find is the following:

'The term climate usually brings to mind the idea of an average regime of weather; 'weather' refers to the condition of the atmosphere at a particular place and time. It is useful to view climate as a system comprising properties and processes which are responsible for its many types of variation, from yearly fluctuations to changes on a millennial time-scale. The most important influence on climatic change is the circulation of the atmosphere.'

This will not do. We all have a vague feeling that we know what climate is, and that, for example, the climates of Manchester, Moscow and Madras are today very different from each other, and that the climate of Manchester today is very different from what it was 20 000 years ago. Such vague feelings can be and have been systematized: an excellent discussion is given in the National Academy of Sciences publication 'Understanding Climatic Change' (Washington, 1975) in Chapter 3. Dr Parry brings out more than once in passing that there is more to climate and the way in which it influences crop yields than simple annual averages of temperature and rainfall, and that the distributions of weather elements within the year and their mutual correlations are very important; such matters should have been analysed thoroughly at the very beginning.

Dr Parry rightly concentrates his attention on areas that have marginal agricultural climates; changes of climate have much more dramatic effects in such areas than in others. He has made a particular study of the Lammermuir Hills and applies the results obtained to the whole of north-west Europe in a very plausible fashion by showing how isopleths of measures of insolation, exposure and summer wetness reveal the patterns of cultivation limits and frequency of crop failure. (It is perhaps worth remarking that the idea of a definite 'growth threshold' for accumulated temperature, whether put at 6 °C or 4.4 °C, has been superseded by recent work which has shown that growth continues, albeit at reduced rates, at temperatures only a little above freezing; however, the patterns of useful growth derived using fixed thresholds are not likely to be altered in their general aspect by such considerations.) Dr Parry's idea of an end of summer 'potential water surplus'—i.e. the excess of a middle and late summer surplus of rainfall minus evapotranspiration—is a useful one for consideration of the effect on harvests of excessive soil wetness; this effect is particularly marked in marginal upland areas. It is, however, *actual* evapotranspiration that should be considered, not *potential*, otherwise in dry sunny summers (e.g. 1976) absurd results would be obtained. Again, soil wetness as such may be of much less importance at harvest time than untimely rain and strong winds that produce 'lodging' and sprouting of grain in the ear. Dr Parry also has a grossly over-optimistic view of the accuracy of evaporation estimates (p. 78):  $\pm 20$  mm is nearer the mark than  $\pm 5$  mm!

The book contains a large number of references to the work of other researchers—climatologists, agricultural economists, geographers, botanists and historians. The conclusions of some climatologists

are sometimes quoted rather uncritically, and several diagrams showing 'running means' are reproduced with no comment on the fallacious and misleading impression conveyed to the innocent reader by this regrettable practice. The style is resolutely undogmatic, however, and a high proportion of sentences contain phrases such as 'seem to bear this out', 'may have occurred', 'probably became', 'suggests an effect consistent with', and so on. The author nevertheless castigates some medieval historians for relying on accounts of climatic change that have long been superseded, and some others for ignoring the subject completely.

The book is written in a clear and attractive manner; it is, however, sad to see the author adopting the barbaric misuse of 'anthropogenic' to mean 'man-made'.

R. P. W. Lewis

*Die Wissenschaft vom Wetter (Verständliche Wissenschaft Band 94)*, by H. Reuter. 185 mm × 115 mm, pp. viii + 146, illus. Springer-Verlag, Berlin, Heidelberg, New York, 1978 (second edition). Price DM 12, US \$6.00.

When the first edition of this book was reviewed in the *Meteorological Magazine* in 1969 almost the only adverse comment made was that the methods of numerical forecasting then in use in several countries were inadequately covered. This edition devotes seven pages to the topic but it must be admitted that this is still a somewhat sketchy account, though probably all that is warranted in an elementary text.

I agree with the reviewer of the first edition that the book otherwise gives 'a very readable non-mathematical account of many aspects of meteorology'. The author discusses most topics in a very open and frank way, not pretending that there are no outstanding difficulties in explaining atmospheric phenomena. This leads him at times into some deep waters where he offers the following piece of metaphysics: Upward motion (of air) is a consequence of cyclonic development but downward motion is a cause of anticyclonic development. After a discussion of several aspects of anticyclonic structure he concludes 'it must be admitted that certain details of the formation of anticyclones require further elucidation.' I personally think that such a frank avowal is to be welcomed even (and perhaps especially) in an elementary text for everyman.

The diagrams and illustrations are with two exceptions (clouds reproduced from WMO Cloud Atlas) clear and the printing is admirable.

M. K. Miles

## Notes and news

### Investiture of Mr C. Kyriacou with the B.E.M.

At a ceremony at Air House, Episkopi on 3 September 1979, Mr Charalambos Kyriacou, supervisor of the meteorological communication centre at RAF Akrotiri, was invested with the B.E.M. by Major-General W. R. Taylor, Administrator of the British Sovereign Base Areas in Cyprus.

Charlie Kyriacou is very well known to the many members of the Meteorological Office staff who have served in Cyprus. Although the 'Met Comcen', previously associated with the Main Meteorological Office at Episkopi and now integrated with the Akrotiri office, is an RAF unit, Charlie and his staff have always seen themselves as, and have been accepted as, part of the Meteorological Office community in Cyprus.



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NOTICES

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Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd, 24-28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly

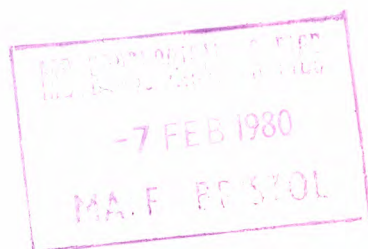
Dd. 586891 K15 12/79

Annual subscription £20.82 including postage

ISBN 0 11 722057 4  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

February 1980

Met.O. 931 No. 1291 Vol. 109

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B4  
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# THE METEOROLOGICAL MAGAZINE

No. 1291, February 1980, Vol. 109

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## **Weather forecasting as a problem in fluid dynamics\***

By Sir John Mason, F.R.S.

(Director-General, Meteorological Office)

### **Summary**

Recent advances in the use of physico-mathematical models to forecast the weather over the northern hemisphere for up to six days ahead are described. A higher-resolution model is used to predict the evolution of smaller-scale weather systems such as fronts and their associated rainfall over western Europe up to 36 hours ahead. An example of a five-day numerical forecast and its verification is presented.

The concepts of precision, accuracy, assessment and evaluation of forecasts are discussed together with the factors that ultimately limit their accuracy and range. The improvement in both surface and upper-air forecasts since the introduction of computer models is demonstrated by both objective tests and more subjective judgements.

### **1. Introduction**

During the last decade the traditional, empirical and largely subjective methods of weather forecasting that depend heavily on the experience, skill and judgement of the individual human forecaster have gradually given way to objective mathematical predictions made with the help of powerful electronic digital computers. The whole operation, described in some detail by Mason (1973), consists of three stages: data acquisition and processing, analysis, and prediction, and consists basically of forming a three-dimensional representation of the conditions prevailing through a large volume of the atmosphere at a particular moment of time, and of predicting, from an observed initial state, the future evolution and movement of atmospheric disturbances and their associated weather. Using standardized observations and measurements, made simultaneously at fixed times over a large part of a continent or even a hemisphere, and exchanged rapidly between different countries in universally agreed codes over a special global network of satellite, cable, radio- and picture-transmission channels, charts are constructed to depict the current distribution of atmospheric pressure, temperature, humidity, winds, etc. at the earth's surface and at a number of levels in the upper air, and from these are evolved forecast charts showing the conditions expected some hours or days later.

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\*Previously published in *Contemporary Physics*, 20, 1979, No. 3, 315–335.



## 2. Description of weather prediction models

The numerical predictions are objective, mathematical exercises based on a firm structure of physical theory that treats the atmosphere as a vast, turbulent, rotating fluid with energy sources and sinks. They involve the construction of physico-mathematical models which, although necessarily simplified compared with the complexity of the real atmosphere, must nevertheless adequately represent the physical and dynamical processes that are likely to control developments on the space and time scales of interest. In other words, the models must properly represent the relevant or significant scales of motion and their non-linear interactions, but smooth out all the smaller-scale motions that cannot be adequately observed or represented individually while allowing for their overall contribution to transport and energy-conversion processes by representing their statistically averaged properties in terms of larger-scale parameters that can be measured. The theory is based on the physical principles of conservation of momentum, mass, energy and water substance, the Newtonian (Navier–Stokes) equations of motions applied to a parcel of air, the laws of thermodynamics, and the equation of state of a gas.

If we consider, at first, a dry atmosphere containing no water substance, the full set of governing equations is as follows:

*Equations (1) and (2).* Two equations describing the horizontal motions of the air in which the time rates of change of the E–W and N–S components of the wind are related to the forces exerted on the air by the rotation of the earth, by horizontal pressure gradients, and by retarding forces such as friction and turbulence.

*Equation (3).* A similar equation describing the vertical motion of the air under the influence of forces that arise from gravity, vertical pressure gradients, rotation of the earth, and from frictional and turbulent stresses.

*Equation (4).* An equation of continuity which relates changes in the density and velocity of the air in such a way that mass is everywhere conserved.

*Equation (5).* A thermodynamic equation which relates the supply of heat to a parcel of air to the resultant changes of temperature and pressure.

*Equation (6).* An equation of state connecting the pressure, density and temperature of the air.

This set of six equations, which may be found in Mason (1971), involves six dependent variables: the three components of the wind, and the pressure, density and temperature of the air, all expressed as functions of space and time. In order to include the effects of evaporation, condensation and precipitation of moisture, equations are added for the continuity of the water substance and the heating term is modified to include the release of latent heat.

Starting from a given initial situation and specified boundary conditions, the problem is to solve a system of simultaneous non-linear partial differential equations in three spatial dimensions with time as the fourth independent variable. In fact, the equations are formulated in such a manner that they allow the time variations of the above quantities to be determined from their spatial variations. Thus, in principle, if we can observe the initial values of all the variables at a network of discrete points filling the whole or a large part of the atmosphere, we can compute the initial time rate of change of each variable from the governing equations, and then extrapolate over a short time interval to find a *predicted value* at each point in the network. Repeating this process step by step, we can build up a forecast of the fields of pressure, wind, temperature, humidity, etc.

In practice, the meteorologist measures the horizontal winds (by tracking balloons with radar), and the atmospheric pressure, temperature and humidity, but not the vertical component of the air motion since this is usually too small to measure directly although of the greatest importance in controlling cloud- and rain-forming processes. In order to get over this difficulty and, at the same time, eliminate



the density of the air as a variable from the equations, we use pressure rather than height as the vertical co-ordinate, regarding this as an independent variable and introduce, as a dependent variable, the contour height, which is the height of a constant pressure (isobaric) surface. Maps of contour heights are very similar, in configuration and in their relation to the winds, to those of the corresponding pressure fields. The thermal structure of the atmosphere is described in terms of the thickness of the layers between isobaric surfaces, the thickness being proportional to the average temperature of the layer. In this system, the vertical velocity of the air is represented by the time rate of change of atmospheric pressure, and is defined entirely by the horizontal wind field. Further simplification is introduced by rewriting the thermodynamic equation in a form that relates the vertical motion to changes in the non-adiabatic heating and the thickness of the layer, so that the dependent variables become the two horizontal components of the wind, the time rate of change of the atmospheric pressure (closely related to the vertical motion), the contour height, thickness, and humidity of the layers between isobaric surfaces, and the precipitated water (rain and snow).

Such complex physico-mathematical models have been developed in the Meteorological Office and described by BurrIDGE and Gadd (1977) to replace a simpler model which formed the basis of its daily forecasting operations between 1965 and 1972.

The current model is designed to simulate and predict the evolution of major weather systems over practically the whole of the northern hemisphere for several days ahead. The basic governing equations are essentially those described earlier and include those for the continuity of moisture. If a layer of air is unsaturated, the humidity is allowed to change by horizontal and vertical air motions and by evaporation of rain or snow falling into it from above. Once a layer reaches saturation, its excess moisture is deemed to condense and fall out as rain or snow (depending upon the temperature) into the layer below. The latent heat released or absorbed during the processes of condensation, evaporation, freezing and melting, is calculated and its dynamical consequences computed. The model allows for modification of the airflow by the underlying topography, for the frictional drag of the land and sea on the air and for horizontal eddy diffusion. Adjustments are also made to allow for the vertical transport of heat and moisture by both shallow and deep convective clouds that are smaller than the computational grid. In computing the exchanges of sensible and latent heat at the earth's surface, the surface albedo is specified at each grid point in terms of the climatic conditions and the snow or ice cover, and sea surface temperatures are held at their monthly mean values. Even for short-period forecasts of two to three days, it is necessary to compute the heat lost by the atmosphere through long-wave radiation to space and the long-wave transfer between model layers including the effects of clouds and the ground, otherwise predicted temperatures for the middle and upper levels are too high.

In addition to this hemispheric model, the Meteorological Office runs a finer-mesh model, described by Bushby and Timpson (1967), Benwell and Timpson (1968) and Benwell *et al.* (1971), with very similar dynamics and physics but with a horizontal grid length of only 100 km limited to a 6400 km  $\times$  4800 km rectangle centred on the British Isles. This provides more detailed forecasts for the United Kingdom and western Europe up to 36 hours ahead, is particularly useful in making quantitative predictions of rainfall from depressions and fronts, and distinguishes between widespread persistent rain and showery convective rainfall. Since the time-steps of the integration are one-third of those for the hemispheric model, the amount of computation, about  $10^{10}$  numerical operations, is about the same, and takes about 10 minutes on the IBM 360/195 computer.

Both models are run twice a day using the noon and midnight observations according to the following operational sequence. When the weather observations from practically the whole of the northern hemisphere, amounting to about one million coded groups per day, arrive at Bracknell, they are automatically checked, quality-controlled, corrected, edited and arranged in suitable format for input to the

models by two dedicated smaller computers. The next stage is to produce analyses of the basic data and from these establish initial values of the dependent variables in the forecast equations. This involves the production, by various objective smoothing, curve- and plane-fitting techniques, of smooth continuous fields of contour heights and humidity mixing ratios from which can be extracted grid-point values over the whole area and for the 10 levels. The analysed fields are now subjected to an 'initialization' procedure to ensure consistency between the contour and wind fields. This involves the solution of two equations, one dealing with the non-divergent component of the wind field and the other with the divergent component and hence the vertical motion, to produce 'best' initial values of the contour height and the three wind components. These parameters together with grid-point values extracted from a smoothed initial humidity field are sorted in column format together with geographical, topographical and climatic constants, surface temperature and humidity parameters, to provide the starting conditions for step-wise integration of the predictive equations.

### 3. Example of a five-day numerical forecast

By way of example, we shall now describe a series of computer forecasts predicting the marked change of weather that took place over the British Isles on 6 and 7 January 1979 when the spell of very cold weather that began on 30 December 1978 gave way to a rapid thaw. This change was well predicted by the hemispheric model from 3 January and the further westerly progress of milder air into north-east Russia during the following two days was also well forecast.

Figure 1 shows the surface weather map based on observations made at 12 GMT, 3 January 1979 when the weather was still very cold over the whole of the British Isles except north-west Scotland. Even the south-south-easterly winds over south-west England were very cold. There was no westerly flow over the Atlantic Ocean to bring milder maritime air into the country. The 24 h forecast valid for midday on 4 January—see Figures 2 (a)–(b)—indicated correctly that the depression to the south-west of the British Isles would move into France (its centre being very accurately placed with the central pressure predicted as 984 mb compared with the actual value of 980 mb), producing strong easterly winds on its northern flank. The mid-Atlantic ridge of high pressure was predicted to move eastwards and the deep depression over Labrador to move north-eastwards with the westerly airstream on its southern flank extending into the Atlantic.

Figure 3 (a) shows that by 12 GMT on 6 January westerly winds had reached most of the British Isles and within 24 h the thaw was under way. The 72 h forecast for the time, Figure 3 (b), indicated a breakdown of the mid-Atlantic ridge allowing the westerly winds with their associated troughs of low pressure to progress and cover the northern half of the British Isles. Although this forecast did not anticipate the development of the anticyclone over continental Europe and of the depression over the Mediterranean particularly well, it gave a good indication of the breakdown of the cold weather regime. Figure 4 (a), a chart of the actual situation at 12 GMT on 8 January, shows that by this time the westerlies and associated troughs covered the North Atlantic and the whole of Scandinavia whilst a new depression was approaching Newfoundland from the west. These developments were all indicated in the forecast made five days earlier depicted in Figure 4 (b) which is the 120 h forecast for 12 GMT, 8 January, based on observations made at 12 GMT on 3 January.

### 4. Precision and accuracy

The precision to be attempted and the accuracy likely to be achieved in predicting the location and timing of a particular weather feature will be dependent upon the time range of the prediction, on the scale and life-time of the weather systems involved, and on the spatial resolution of the forecasting

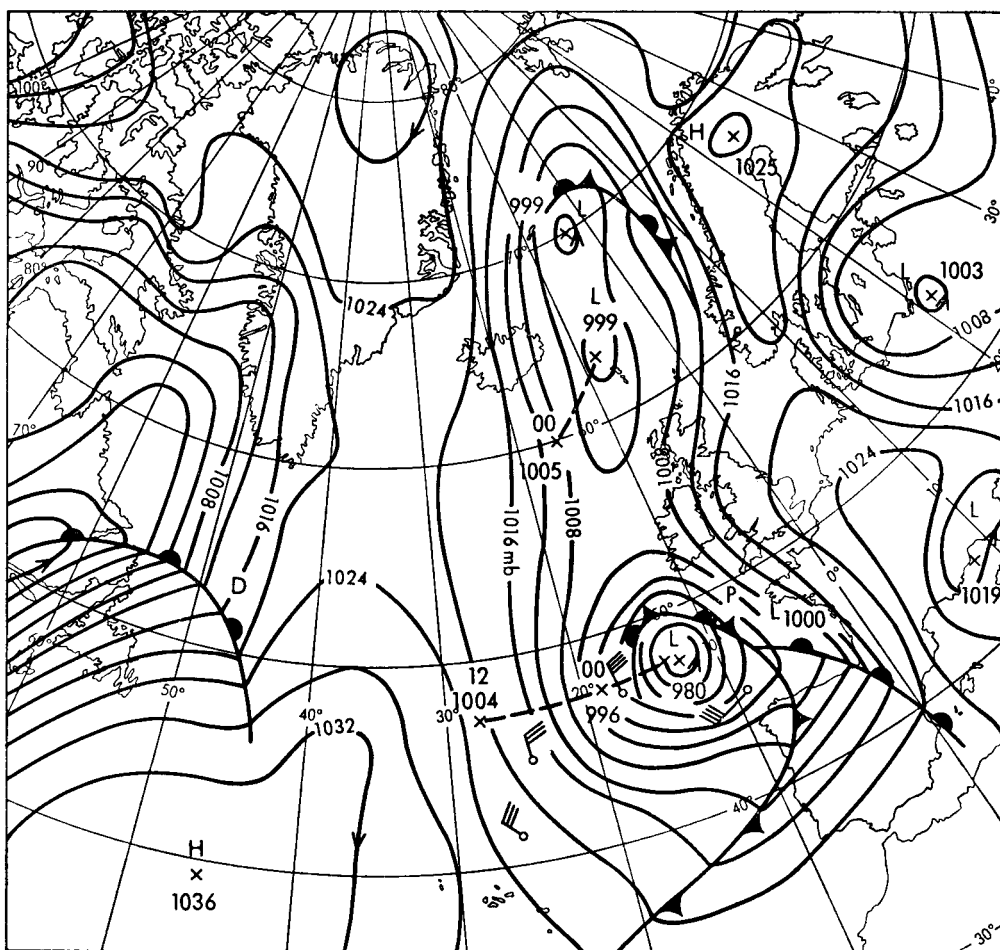


Figure 1. The actual surface weather map drawn from observations made at 12 GMT on 3 January 1979. All the forecasts shown in subsequent diagrams are based on the observations made at this time.

model. Thus the fine-mesh model described earlier will predict the geographical position and central pressure of a major cyclone 1000 km in diameter and lasting for five to six days within 50 km and 2 mb respectively. For a forecast made for only 24 hours ahead it is reasonable to attempt this degree of precision in relation to the accuracy achieved. However, the errors grow roughly exponentially as the forecast period is extended, so that although the model will continue to produce a deterministic and apparently precise forecast, precision and accuracy progressively diverge, and beyond five to six days the accuracy at the present time usually falls below useful limits.

At the other end of the scale, prediction of the location, movement and intensity of individual shower clouds, only 1–10 km in diameter and lasting for <1 hour, is impossible with present models which lack the requisite small-scale physics, observations and resolution. The best that can be done is to predict where and when the atmosphere is likely to become convectively unstable within a layer of

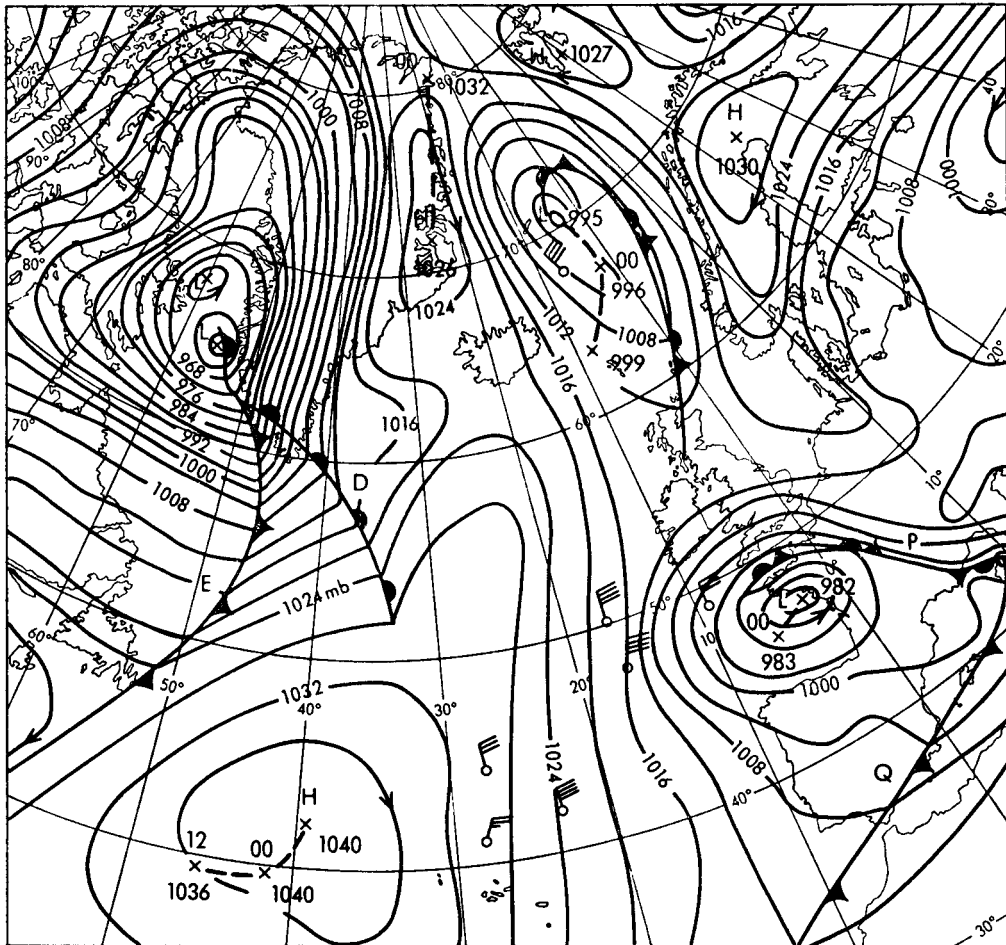


Figure 2(a). Actual surface weather map for 12 GMT, 4 January 1979.

restricted depth and so liable to sporadic outbreaks of showers, but there is no possibility of predicting exactly where and when a shower will actually appear. The forecast may then perform take the form of say, 'a high risk of scattered showers, mostly light and of short duration' since they are likely to be almost randomly distributed. Indeed, a shower having been accurately located by radar or satellite, its subsequent motion and development can best be predicted by extrapolation, not necessarily linear extrapolation, of its recent evolution.

In summary, it is not possible, by the intrinsic nature of the problem, to predict accurately the location of a small weather system for more than a short time in advance: precision may be possible at the expense of range, but a long-range forecast will necessarily be lacking in precision and detail. It is just not possible to achieve both precision and range in the same forecast.

The achievable accuracy for prediction on a particular time and space scale is limited by

(i) inadequacies in the coverage, frequency, accuracy and representativity of the observations used to define the initial state;

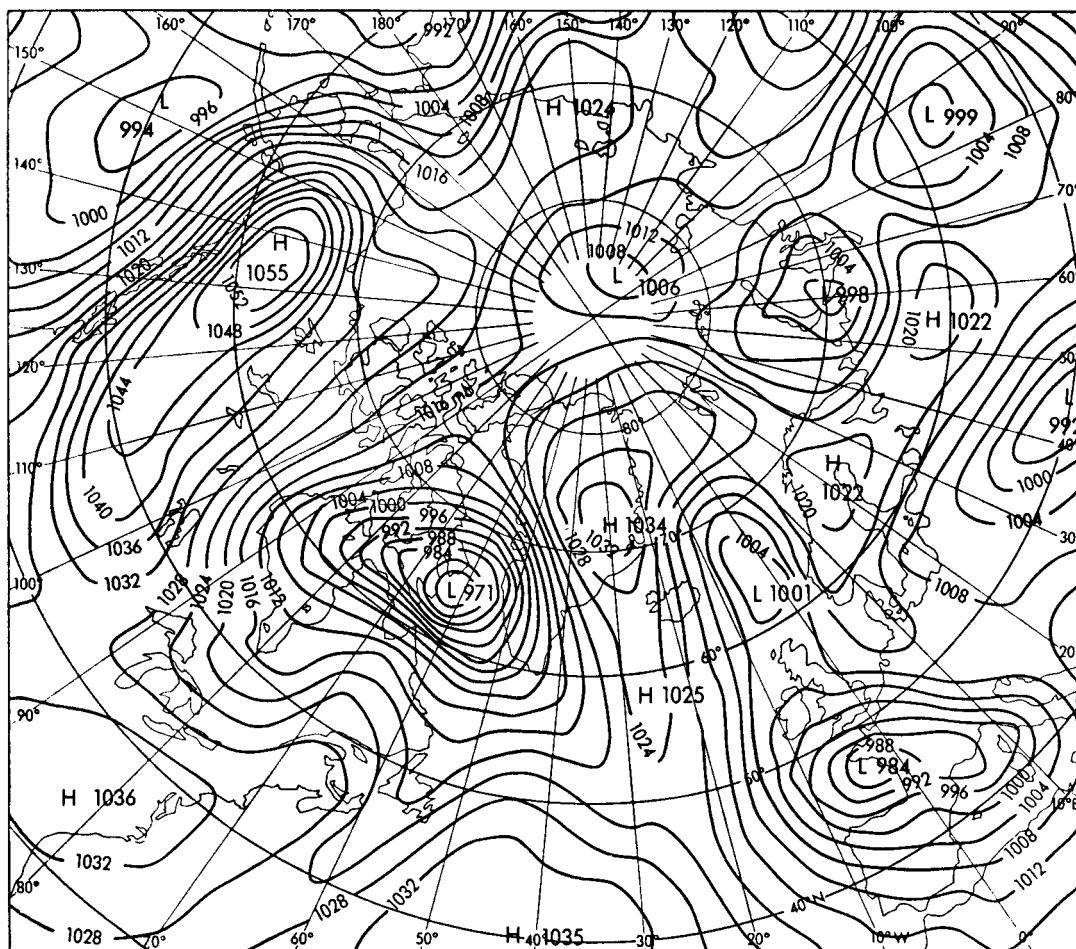


Figure 2(b). 24 h computer forecast for 12 GMT, 4 January 1979.

- (ii) neglect in the models of some of the physical processes, especially small-scale processes, or their inadequate statistical representation;
- (iii) computational errors which arise at each time-step of the integration and so build up cumulatively;
- (iv) random fluctuations in the real turbulent atmosphere which are not represented in the model.

Although the models themselves are far from perfect, serious errors in short-range (one- to five-day) weather predictions probably arise mainly from the lack of adequate observations, especially from over the oceans and remote land areas. Accurate longer-range predictions for a week or more ahead may not be possible without allowing for interaction between the atmosphere and at least the surface layers of the oceans that may produce anomalies in sea surface temperature relative to the long-term average values. As to model deficiencies, it is difficult to distinguish between the effects of numerical errors introduced by the finite-difference approximations to the continuous dynamical equations and those

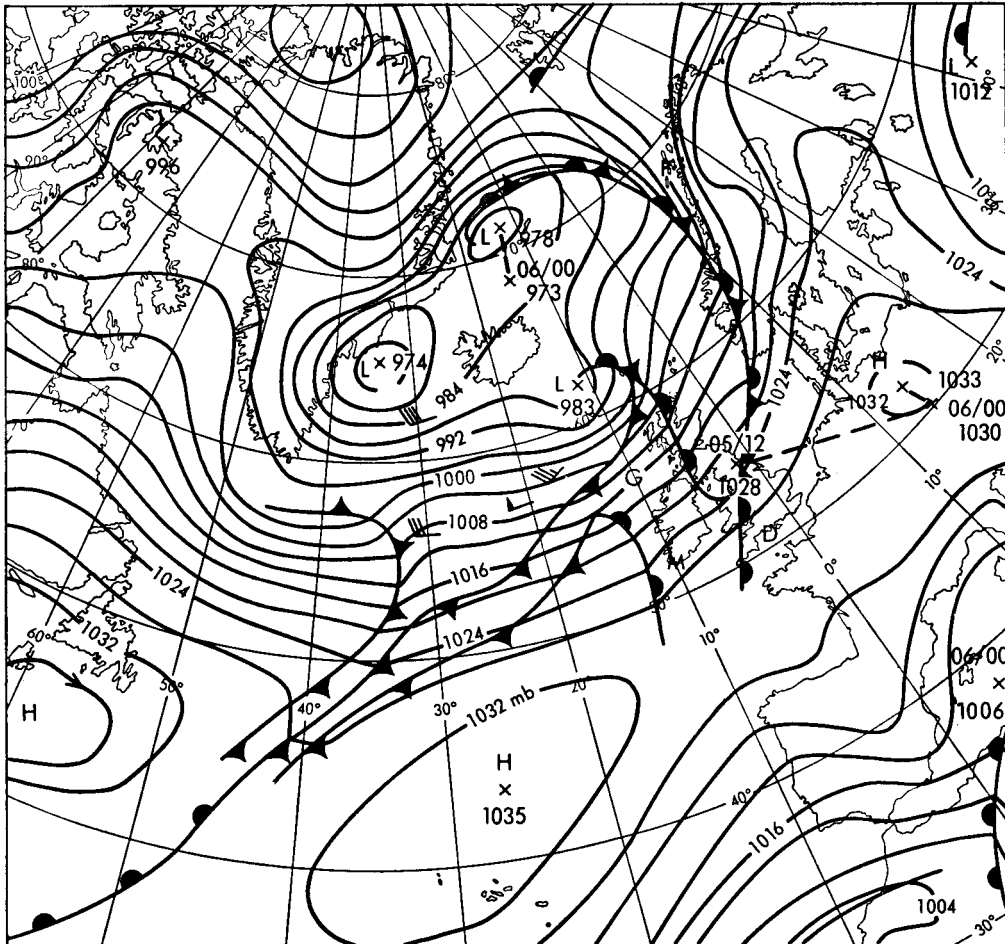


Figure 3(a). Actual surface weather map for 12 GMT, 6 January 1979.

that arise because the equations themselves do not provide a complete and exact description of atmospheric behaviour, but the two sources of error appear to be of comparable importance. Some of the more important deficiencies arise from inadequate horizontal and vertical resolution and from inadequate representation of topography, precipitation, convection, horizontal eddy viscosity and surface drag. All require a good deal more research.

## 5. Verification and evaluation

While there is naturally much public interest in the accuracy of weather forecasts, their utility and economic value excites less comment, yet both accuracy and usefulness are important if forecasts are to be used to maximum advantage. It is convenient to distinguish between *verification* in which the emphasis is on the degree of goodness of the forecasts when compared with the actual weather during the forecast period, and *operational evaluation* in which the forecasts are judged in terms of their value

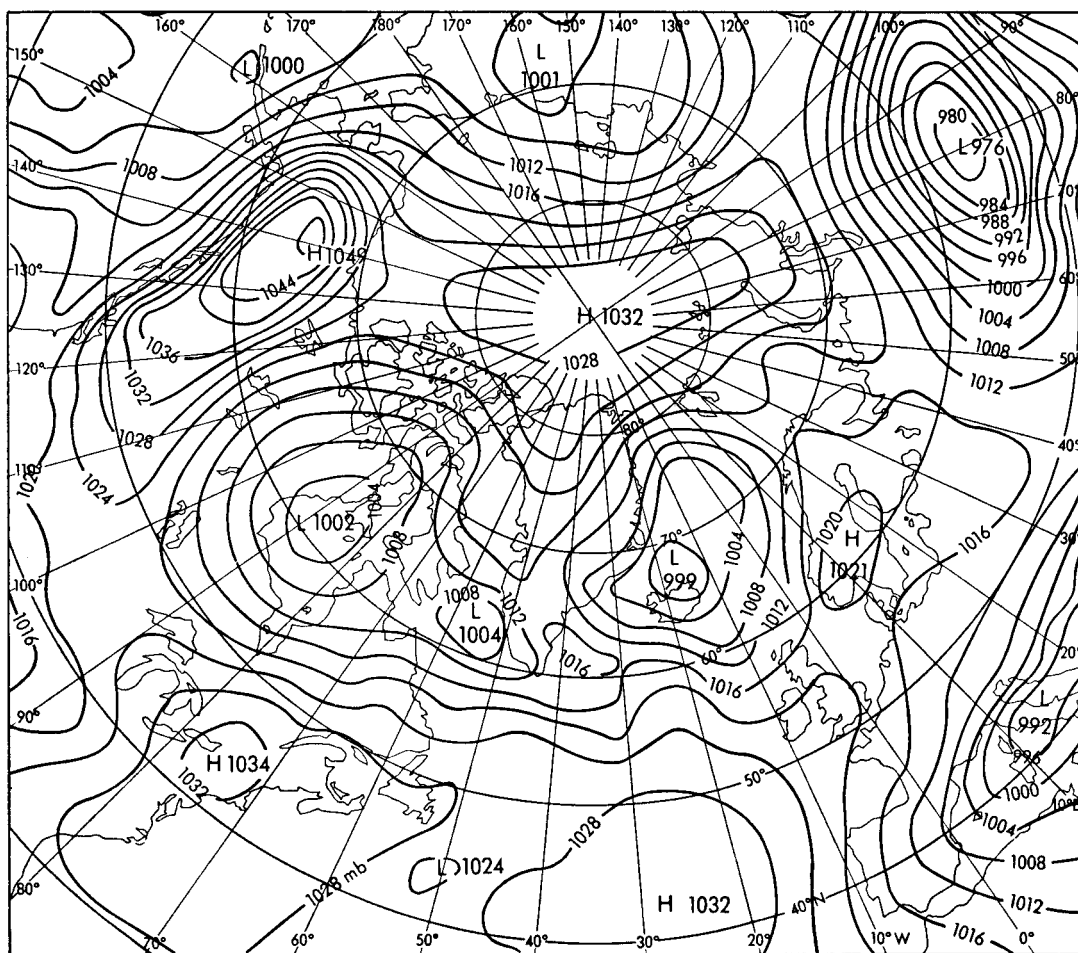


Figure 3(b). 72 h computer forecast for 12 GMT, 6 January 1979.

or utility to the user. Verification of forecasts is undertaken by the Meteorological Office as a routine activity to provide an overall measure of the performance and effectiveness of the prediction models and techniques and to monitor the effects of changes introduced from time to time.

Many different types of forecast are issued to serve a wide variety of customers. Forecasts for the general public, disseminated mainly by television, radio and the press, are largely expressed in words. The two million forecasts made each year for civil and military aviation, and the tailor-made forecasts for such weather-sensitive industries as electricity, gas, shipping, and offshore oil and gas are usually presented in numerical form or by specially prepared charts. Meteorologists themselves make great use of prognostic charts, often the output of the numerical models and drawn by computer, of the expected distributions of atmospheric pressure, temperature, winds, rainfall, etc. An assessment of their accuracy can be made by comparison of forecast and actual values at a three-dimensional network of grid points and is an objective and fairly straightforward exercise. Forecasts expressed in words have to be assessed

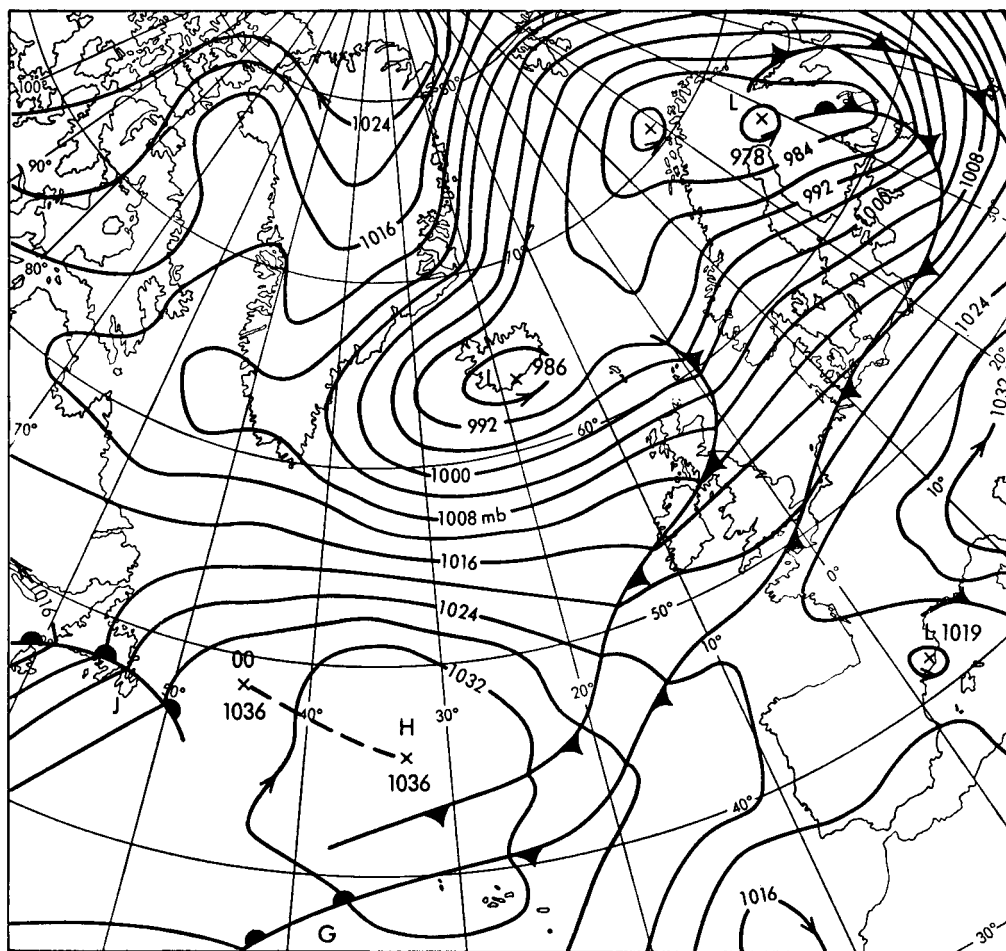


Figure 4(a). Actual surface weather map for 12 GMT, 8 January 1979.

subjectively and the results are inevitably less precise and more difficult to interpret. In an attempt to bridge the gap the Meteorological Office is conducting some experiments in which a computer is programmed to produce objective, worded forecasts from the output of the numerical prediction models.

A decade ago, before the introduction of advanced computer models, forecasts were rarely issued for more than 24 hours ahead and were less detailed and less accurate than those of today. The numerical methods have led to a greater degree of continuity, consistency and confidence in the forecasts than existed when they depended entirely on the personal experience and judgement of changing rosters of forecasters. Perhaps the single largest contribution of the numerical models has been to extend the range of surface forecasts from one to three days and to provide useful guidance up to six days ahead.

In the objective assessment of numerical forecasts, much attention is given to the forecast chart of the distribution of atmospheric pressure at the earth's surface on which weather forecasts for the general public are largely based. One standard test is to compare the predicted height of the 1000 mb pressure



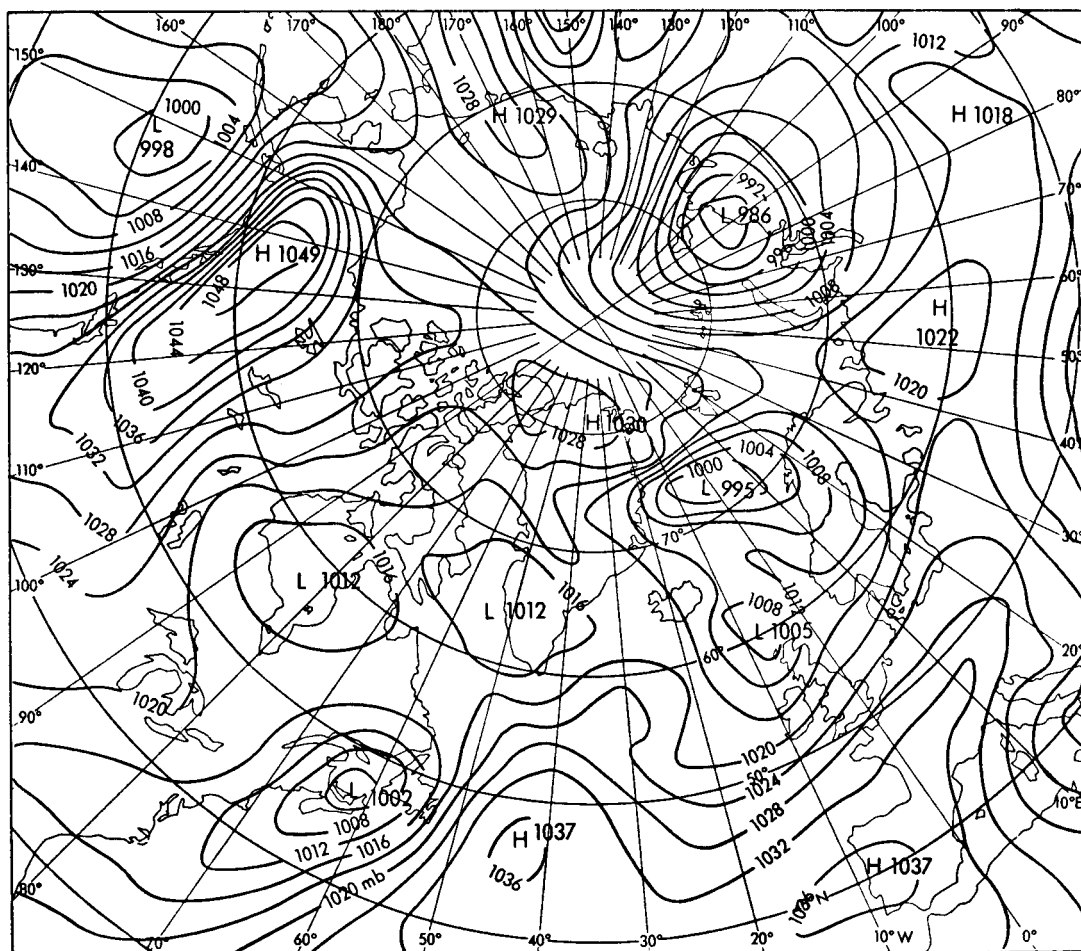


Figure 4(b). 120 h computer forecast for 12 GMT, 8 January 1979.

surface at a large number of grid points with the corresponding heights on the 'actual' or verifying chart and then calculate the mean height error or 'bias' and the root mean square (r.m.s.) height error. Since it is a simple matter to derive the geostrophic winds from the pressure distribution, the r.m.s. vector wind error, which takes account of errors in both speed and direction of the wind, is also calculated\*. The results of the analysis, some of which are shown in Figure 5, indicate that the mean height error or bias has now been almost entirely eliminated in surface forecasts up to 72 hours ahead whilst the r.m.s. height and vector wind errors have been substantially reduced so that the 72 h and 48 h forecasts are now about as good in these respects as the 48 h and 24 h forecasts were a decade ago. The statistics also show a marked improvement in forecast performance soon after the introduction of the

\*This is a stringent test because, for example, a forecast wind of 30 kn ( $\approx 15 \text{ m s}^{-1}$ ), exact in magnitude but with a  $30^\circ$  error in direction, would have a vector error of 15 kn ( $\approx 7.5 \text{ m s}^{-1}$ ).

ten-level models in August 1972 and a continuing steady improvement ever since. Figure 5 shows that the correlation coefficient between the forecast and observed 72 h changes in the height of the 1000 mb pressure surface increased from only 0.32 to 0.65 during 1972/73 and has since increased to 0.72, whilst the correlation for 24 h changes has improved from 0.77 to 0.85.

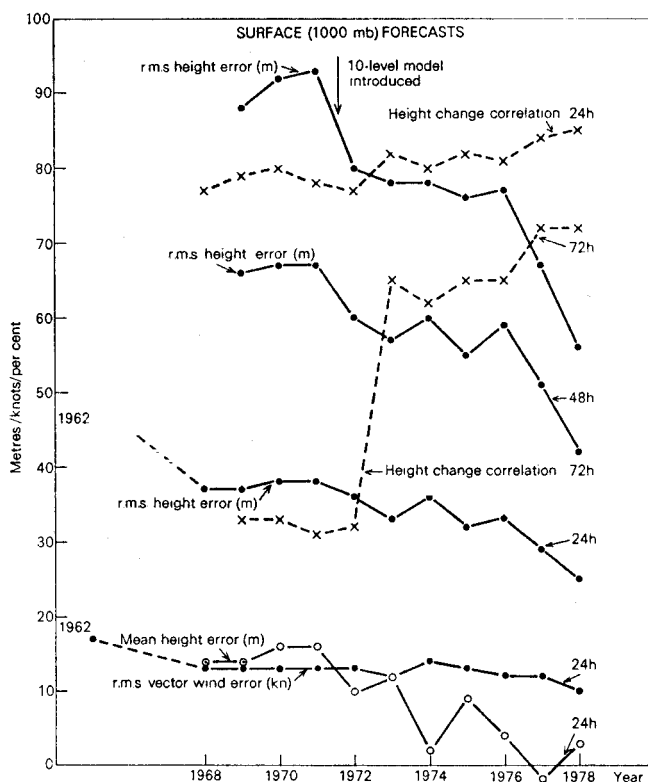


Figure 5. Errors in numerical forecasts of the height of the 1000 mb pressure surface and of the geostrophic winds, showing continual improvement over the last decade.

Predictions of air temperature made 24 h in advance for the 1000 mb pressure surface of the hemispheric ten-level model show r.m.s. errors of about 3 K. However, these predictions may be improved by allowing for additional factors such as cloud cover and local effects so that 90 per cent of the 12 h forecasts and 80 per cent of the 24 h forecasts issued to the Gas and Electricity Boards are correct within  $\pm 2$  K, only 1 per cent and 4 per cent respectively being in error by more than 5 K.

Even the first operational numerical three-level model introduced in November 1965 proved markedly superior to subjective methods in forecasting winds and temperatures at upper levels for aviation and soon thereafter all such forecasts were based on computer methods. Numerical values of the forecast parameters are fed directly from the Meteorological Office computer into the airline computers for flight planning and also provide the basis of the meteorological advice (flight documentation) issued to

aircrews. The numerical methods have led to the r.m.s. vector errors in the 24 h forecasts of winds at 500 mb (18 000 ft) and 200 mb (40 000 ft) being halved from about 30 kn to about 15 kn. The 48 h forecast errors of about 20 kn and the 72 h errors of about 25 kn are now very little larger than those of the corresponding 24 h and 48 h forecasts only five years ago. Similar improvements appear in the r.m.s. height errors and the mean height errors have now been reduced to only a few metres. Again, as shown in Figure 6, marked improvements followed the introduction of the 10-level model which also produces better forecasts of the location and strength of the high-level jet streams.

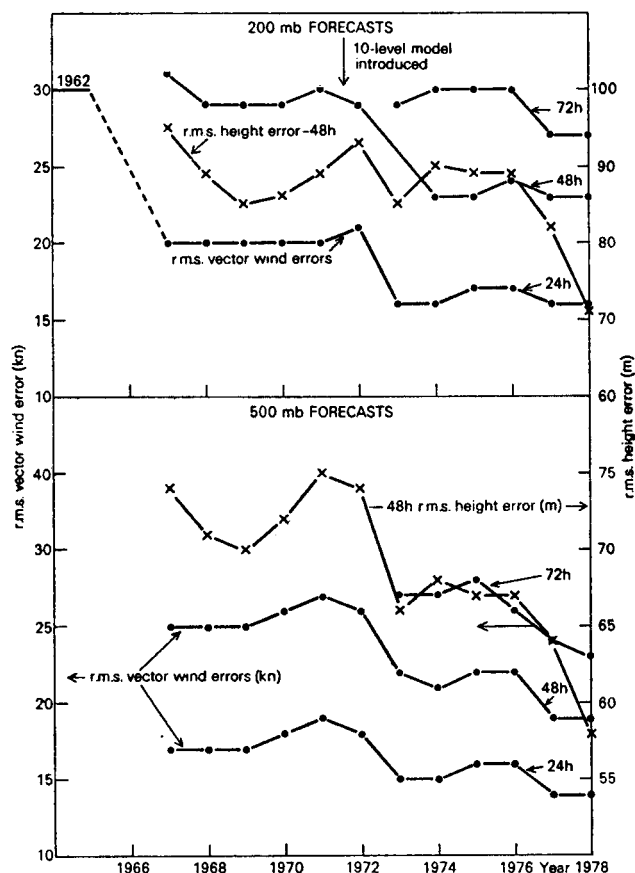


Figure 6. Errors in numerical forecasts of winds at 500 mb ( $\approx 18\,000$  ft) and 200 mb ( $\approx 40\,000$  ft) and in 48 h forecasts of the height of those pressure surfaces.

Desirable standards of forecasting accuracy for civil aviation are set by the International Civil Aviation Organization (ICAO). They are: upper winds (at levels above 25 000 ft) within 20 kn vector difference on 90 per cent of occasions over a period of one hour's flying time and upper-air temperatures

within  $\pm 3$  K on 90 per cent of occasions. The average r.m.s. errors in the 24 h forecasts issued in 1978 were only 2 K and 14 kn at 500 mb and 3.5 K and 16 kn at 200 mb, and since errors evaluated along 500-mile tracks (equivalent to about one hour's flying) are smaller than those for spot values at grid points, it appears that the ICAO requirements are now being met except perhaps in the vicinity of jet streams and areas of sharply changing gradients.

For longer-range model predictions, for up to six days ahead, the r.m.s. vector errors in the forecast winds at 500 mb, which average 16 kn after 24 hours, double during the next three days but are still below the errors of forecasts based on persistence after six days. A similar performance is achieved at the 1000 mb and 200 mb levels.

Whereas objective tests can be devised to assess the accuracy of numerical forecasts, it is much more difficult to assess written and spoken forecasts, which are largely descriptive, and here the approach has to be more subjective. The difficulty lies in deciding what weights should be given to the various elements of the forecast in arriving at an overall figure of merit for the forecast as a whole. Although it is possible to design sensible systems of awarding 'skill-scores' for individual parameters such as temperature, wind, precipitation, etc., it is much more difficult to combine these individual marks into an overall score for the forecast as a whole. This is mainly because the weighting of the various factors will be judged differently by different users and should probably vary with season and climatic regime. Thus a forecast which gets high marks for temperature and wind but is a few hours out in predicting the arrival of a rain belt may be regarded as a complete failure by a farmer concerned with getting in the harvest whilst a yachtsman or a power engineer may judge it more leniently. Or, to take another example, an error of a few degrees in the forecast minimum temperature may be unimportant and pass almost unnoticed in midsummer, but a similar error may have very serious consequences when the temperature is near freezing.

Since August 1963 the Meteorological Office has applied a simple marking scheme to the 0755 BBC forecast valid for the following 16 hours and the 1755 forecast valid for the following 30 hours. The forecasts for each of the seven regions are checked by selected Meteorological Office stations within the region. Each forecast is evaluated in terms of four parameters—wind speed and direction, weather (precipitation, fog, etc.), state of sky, and maximum temperature—each being awarded marks 2, 1, or 0 according to whether the forecast is regarded as correct, only partly correct, or incorrect. Since the scores turn out to be much the same for the four elements, an average mark is taken as a measure of the overall performance and avoids the difficulties of assigning different weighting factors to the various elements. Although the assessments are made at many different stations and by many individuals, there is a high degree of uniformity between the different regions. The 0755 forecast, as might be expected, is consistently more reliable than the 1755 forecast, the overall scores for 1978 being 86 and 78 per cent respectively.

These forecasts are based on guidance given by the Central Forecasting Office at Bracknell in country-wide 'synoptic reviews' issued some 2½ hours earlier. They, in turn, depend heavily on the predictions of the numerical models and have shown marked improvements in recent years. Whereas 10 years ago less than half of these guidance forecasts were essentially correct and 1 in 7 were seriously misleading, since 1972 two-thirds have given good guidance and only 1 in 15 has contained serious errors.

The overall improvement in the two- and three-day surface weather forecasts in recent years, which again are largely based on the numerical prognoses, is apparent from Table I, showing the percentages of all forecasts falling within three performance categories.

Although these recent improvements may be attributed largely to the numerical models, the human forecaster still has a vital role to play in interpreting the computer products, in modifying them if he has good reason, and in predicting the behaviour of small-scale weather systems and local phenomena such

as thunderstorms, showers, fog, frost and ice which are not treated by the models. Indeed the improvements and advantages derived from numerical models may be largely nullified by forecasters lacking in experience and judgement of the real atmosphere.

**Table I.** *Evaluation of 48 h and 72 h surface forecasts.*

	48 h forecast			72 h forecast		
	A	B	C	A	B	C
	<i>per cent</i>			<i>per cent</i>		
1967	24	51	25	7	50	43
1972	51	37	13	27	45	28
1976	53	37	10	27	43	30
1977	59	32	9	31	46	23
1978	63	28	9	36	41	23

A—essentially correct. B—some errors, but none serious over the United Kingdom. C—misleading in some important respect.

## 6. Limits of atmospheric predictability

We now raise the crucial question of whether there is for each scale of atmospheric motion a finite intrinsic time range of predictability beyond which it is not possible to make a deterministic forecast. This question is of fundamental importance for practical weather forecasting because the answer may set ultimate limits to what is achievable and to what is worth aiming at.

Smagorinsky (1969) has posed the question as follows: 'If we had a physically faithful model of the real atmosphere, an ability to specify fully the initial conditions for all spectral components, and committed no truncation error in numerically integrating the system of non-linear differential equations, could we expect to predict the atmospheric evolution from an initial state with infinite precision infinitely distant into the future? Or, would small random perturbations (noise) develop in the model and amplify to the point at which numerical simulation departs from reality and ultimately become uncorrelated with the real atmosphere?' An answer to these questions might set an ultimate time limit to predictability beyond which no improvement, whether in models, initial conditions (observations), or computing power would increase the predictability of a given scale of atmospheric development.

Leading authorities such as Lorenz (1969) and Leith (1971, 1978) hold that a definite limit to predictability is set by the inherent instability of atmospheric motions and by their inherent non-linear and dissipative character. Robinson (1967, 1978) goes further and asserts that the modified Navier-Stokes equations (1-3) cannot be used to predict the mean motion of an atmospheric disturbance of horizontal scale  $L$  for times  $t > L^2/K$  because it will lose its identity through the dissipative action of smaller eddies collectively represented by the lateral eddy diffusion coefficient  $K$ . Arguing on dimensional grounds that  $K \approx L\bar{U}$ , where  $\bar{U}$  is the mean horizontal velocity of the flow, he arrives at a prediction time limit  $t \approx L/\bar{U}$  which is equivalent to saying that the prediction time cannot exceed the time taken for the system to travel about one wavelength. For a middle latitude depression (cyclone) of  $L = 5000$  km travelling at  $U = 20$  m s<sup>-1</sup>, this gives a time limit of *c.* 3 days. Extensive experience with operational forecasting models shows this estimate to be too pessimistic. It is possible to predict the evolution of systems over considerably longer times than it takes to displace them by one wavelength. It is possible to predict major depressions for at least five to six days ahead (see section 3) with even the present imperfect models and inadequate observations. Moreover, the models are able to predict the formation and development of depressions and even fronts which are not present or detectable in the

smoothed fields of the initial state. The unreality of Robinson's treatment is further illustrated by its conclusion that finer resolution of the observational or computational grid would actually *reduce* the predictability time for larger-scale motions whereas experience shows that higher resolution leading to better description and prediction of the smaller scales also leads to improved simulation of the larger scales such as the planetary long waves.

A similar examination of the Navier–Stokes equations has been made by Leith and Kraichnan (1972) but treating the basic flow in spectral form and using a different closure hypothesis with a scale-dependent damping factor that ensures that interactions involving the smaller scales are heavily damped. They then define the predictability time as the time taken for the 'error energy' on any one scale to grow to one-half of the energy of that scale and arrive at periods at least 20 times greater than those of Robinson!

It is important to realize that these and similar analyses, which have attracted much attention and discussion, apply to predictions, from initial values, of the motions of an *unbounded* fluid in which motions of all scales are treated as decaying isotropic turbulence losing energy solely through a cascade of progressively smaller eddies and ultimately by molecular viscosity. Such a system has little in common with the real atmosphere which is bounded and largely forced from the planetary surface containing major energy sources and sinks, and in which quasi-two-dimensional baroclinic disturbances such as the mobile cyclones and anticyclones tend to become stabilized through non-linear interactions with smaller scales, the latter being able to transfer energy to the larger-scale motions as well as dissipate it through small-scale turbulence. Moreover, large-scale forcing of the atmosphere as the result of differential heating, thermal contrasts between land and ocean, the large thermal capacity of the oceans and rotation of the earth tends to impose preferred scales of motion and confer stability especially at longer wavelengths.

For all these reasons atmospheric motions are almost certainly predictable over longer periods than Robinson's highly artificial calculations for an unforced system of decaying isotropic turbulence suggest. It is, however, very difficult to derive a convincing theoretical criterion for the predictability of a system as complex as the real atmosphere, so we have to appeal either to its observed behaviour or to the results of experiments with numerical models.

One approach is to determine from observations for how long the structure and evolution of weather systems are influenced by the initial state. The effects of such persistence may be measured by correlations between two sets of observations made at increasing time intervals apart. Tests made with seven surface and upper-air observed parameters analysed on the spatial grid of the Meteorological Office five-level global model indicate correlation coefficients of about 0.3 for observations made 10 days apart but falling to less than 0.1 for data sets 30 days apart.

Another way of studying predictability is to determine how fast small errors in the representation of the initial state will grow in a numerical model. The method involves running the model from a given set of initial parameters and again after a small random error is added at each grid point and following how rapidly the two simulations or predictions diverge. Experiments of this type with global models usually find that the errors double within a period of 2–4 days and suggest an upper limit of a few weeks for the prediction of day-to-day weather variations. Thus Smagorinsky (1969) reports the result of a test with a nine-level global model with horizontal grid spacing varying from 320 km at the Equator to 640 km at the Poles. Comparison trials were made for an observed (January) initial state and for a perturbed state in which a random temperature fluctuation of standard deviation 0.5 K was added at all grid points at all levels. The standard deviation  $\sigma$  of the temperature error (difference between the predicted temperatures of two runs) grew exponentially during the first seven days with a doubling time of about  $2\frac{1}{2}$  days and thereafter more slowly. Nevertheless, after three weeks,  $\sigma$  was still only

about half the asymptotic value of the standard deviation of the difference between the predicted and initial (unperturbed) temperatures which was 5.5 K and taken as a measure of the natural variability.

This result is rather similar to that obtained for the growth rates of r.m.s. vector wind errors in the Meteorological Office hemispheric forecast model described above, where the errors doubled in the first three days but were still below the persistence values after six days. Some recent tests by Rowntree (personal communication) with a Meteorological Office global five-level model for three winter seasons show that differences in the model atmosphere's evolution induced by changes in the initial data were detectable up to about three weeks later but thereafter any influence of the initial conditions was lost in the random noise. Moreover correlations between *predicted* and *observed* states, both represented by 10-day averages at grid points, fell sharply from about 0.5 for the first 10-day period to about 0.1 for the third period.

All these results tend to indicate an upper limit of about one week for useful deterministic predictions with present models and observations but it is reasonable to hope for an extension to perhaps two to three weeks as both the models and their initial data improve.

## 7. Future outlook

Although forecasts issued to the general public are usually limited to three days ahead, the hemispheric model is run up to six days ahead once a day and the results provide the basis for the weekly farming forecast issued every Sunday. Although the model remains stable for much longer periods, the detailed predictions tend to deteriorate rather sharply beyond the fourth or fifth day. Reliable forecasts for a week or more ahead, which would be of great economic value to weather-sensitive industries such as agriculture, building, fuel, power, water resources and food manufacture, will require improved models with higher resolution, better representation of the physics, especially of clouds and of convective, radiative and turbulent transfer processes, and improved mathematical methods of integrating the equations in order to limit both the amount of computation and the errors involved. Geographical extension of the model, perhaps to cover the whole globe, improved observations from the oceanic and tropical regions, and greater computing power will also be required. There are, however, *inherent* limitations in the predictability of atmospheric behaviour set by the cumulative effect of random, small-scale disturbances that cannot be directly observed or treated explicitly in the models. None the less, current research encourages us to think that it should be possible to forecast the evolution of major weather systems such as the mobile depressions of middle latitudes for about a week or 10 days ahead and to give a useful indication of general trends over periods of perhaps a month. In some recent experiments in which the model equations were integrated up to 50 days and the results averaged over 10-day periods, the development of the long planetary waves, which largely determine the general character of the weather over periods of several weeks, was predicted quite well. A clearer idea of the possibilities should emerge when the recently inaugurated Global Weather Experiment, which is to make a continual, comprehensive survey of the global atmosphere for a period of 18 months, provides the models with input data sets of unprecedented quality and coverage.

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- |  |      |   |
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## Examples of snow prediction using the 10-level model

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### Summary

The output of the fine-mesh (or rectangle) version of the Meteorological Office operational 10-level model was assessed for its value as a snow predictor during the week 9–15 February 1979. The actual weather for the week was compared with that forecast by the model, with particular attention to the predicted probability of snow in relation to the observed change-over from rain to snow over the United Kingdom. A similar comparison was made using forecasts of probability of snow corrected as suggested by Boyden for sea-level pressure and topographic height.

### Introduction

The fine-mesh (rectangle) version of the Meteorological Office numerical forecast model produces forecast fields of various parameters as guidance to the forecaster. Amongst these fields are two that may be useful in predicting snow. These are the precipitation patterns and the 1000–850 mb thickness field. Two isopleths of 1000–850 mb thickness are output and are labelled 20 per cent and 80 per cent. These are the probabilities that the precipitation will fall as snow at sea level according to Boyden (1964).

It is realized that the value of the 1000–850 mb thickness that corresponds to a change from rain to snow will depend on whether the layer up to 850 mb is stable or unstable. A stable easterly airstream may have a temperature near zero degrees Celsius from the surface to 850 mb and produce snow at a relatively high thickness. Conversely a moist, unstable westerly with relatively low thickness may have a temperature of  $-8^{\circ}\text{C}$  at 850 mb and still not produce snow at 1000 mb. Also the height of the wet-bulb freezing level is probably the most reliable predictor of snow as this is more representative of the shallow layer near the ground which is usually the important region for melting or not melting the snow (see for example Lowndes *et al.* 1974). Unfortunately the rather coarse 100 mb layers of the 10-level model do not permit accurate prediction of this variable, so the 1000–850 mb thickness is used.

The week of 9–15 February 1979 was chosen for an investigation of the reliability of the 10-level model as a snow predictor. This was a particularly good week for a test as during this period snow or rain fell every day over a large part of England and Wales. During the first part of the week both forms of precipitation fell over different areas at the same time, allowing a boundary to be identified where a change from snow to rain occurred.

Pressure was low for much of the period to the south-west of Britain and fronts remained slow-moving over south and south-west England. Between the 11th and 14th two depressions moved east over northern France and the Channel, the second depression transferring the main centre of low pressure from south-west to south-east of Britain. This resulted in a return of very cold air to the whole of Britain and severe blizzards in eastern England.

### Method of assessment of the model's snow predictor

The reported present weather over the United Kingdom from 00 GMT on 9 February to 18 GMT on 15 February 1979 was examined for every hour, and areas of snow were separated from those of rain or sleet. The boundaries between these snow areas and the rain areas, referred to as the snow-line in this paper, were compared with the forecast snow probability lines. Thus a snow-line could be identified with an interpolated value of forecast snow probability. This identification was carried out at each

main synoptic hour using the forecasts (i.e.  $T + 12$ ,  $T + 18$ , etc.) valid at that time. This process yielded about 60 values of the forecast snow probability that corresponded with snow-lines during the week in question. Four sets of such values were produced as follows:

(a) A set of values obtained from the operational forecast charts, i.e. using 20 and 80 per cent snow probability lines derived from 1000–850 mb thickness.

(b) A set obtained from charts including a 50 per cent probability line to give a more accurate interpolation than between the 20 and 80 per cent lines of the operational output.

(c) A set obtained by using Boyden's corrections for sea-level pressure applied to the thicknesses (50 per cent probability lines were also drawn on the charts).

(d) A set obtained from charts including a 50 per cent probability line but with Boyden's corrections both for sea-level pressure and for topographic height. The heights used for correcting the thickness are averages for each 100 km square of the rectangle grid.

The fully corrected thickness ( $h_1'$ ) for these charts was then

$$h_1' = h_0' + (h_{10} - H)/30 \quad \dots \quad (1)$$

where  $h_0'$  is the uncorrected 1000–850 mb thickness (as used in sets (a) and (b)),  $h_{10}$  is the height of the 1000 mb surface above sea level, and  $H$  is the height of the ground above sea level. This formula is empirical and was derived by Boyden (1964).

Additionally the forecast snow probabilities were compared with the probabilities obtained from the objective analysis of the 1000–850 mb thickness at 00 and 12 GMT each day to assess the accuracy of the predicted thickness. The forecasts were also examined for their accuracy of precipitation area since obviously one needs precipitation as well as a suitable probability before snow can be forecast.

## Discussion and results

In general the precipitation areas were fairly well forecast during the week, the only inaccurate period being 06–12 GMT on the 11th when most forecasts predicted significant precipitation over the south and south-west of England. In fact there was only light and sporadic rain in this area and very little elsewhere during this period.

It is interesting to note that errors in precipitation forecasts can lead to errors in thickness forecasts. The forecast model (Burrige and Gadd, 1977) includes mechanisms for cooling a layer both by evaporation of precipitation and melting of snow. If, for example, precipitation is forecast by the model and forecast temperatures and humidities are such that either of these cooling effects can occur, the forecast thickness will be too low if in reality no precipitation occurs. In short, an inaccurate forecast of precipitation results in an inaccurate forecast probability of snow.

Figure 1 illustrates this effect quite well. The precipitation which was erroneously forecast to spread north between 06 and 12 GMT on the 11th caused a decrease in forecast thickness and hence an increase in the probability of snow by something in excess of 30 per cent over southern England. Advection considerations would indicate little change in the thickness. The computer analysis for 12 GMT on the 11th (not shown) does in fact show the 20 per cent and 50 per cent probabilities in positions not far removed from the forecast for 06 GMT on the 11th, as would be expected, there having been no significant precipitation during this period. Throughout the week as a whole the contribution due to melting and evaporation of the precipitation appeared to increase the forecast probability by about 30–50 per cent for the forecasts studied, depending on the intensity of the precipitation. By studying the probabilities obtained from the objective analyses during the week and the way in which these were changed by precipitation, a value for the actual change in probability due to precipitation occurring was obtained.



Plate I. His Royal Highness the Prince of Wales and the Director-General of the Meteorological Office in the Central Forecasting Office at Bracknell (see page 64).



Plate II. His Royal Highness the Prince of Wales, Sir John Mason, and Mr D. E. Jones, Assistant Director (Central Forecasting), in the Central Forecasting Office at Bracknell (see page 64).

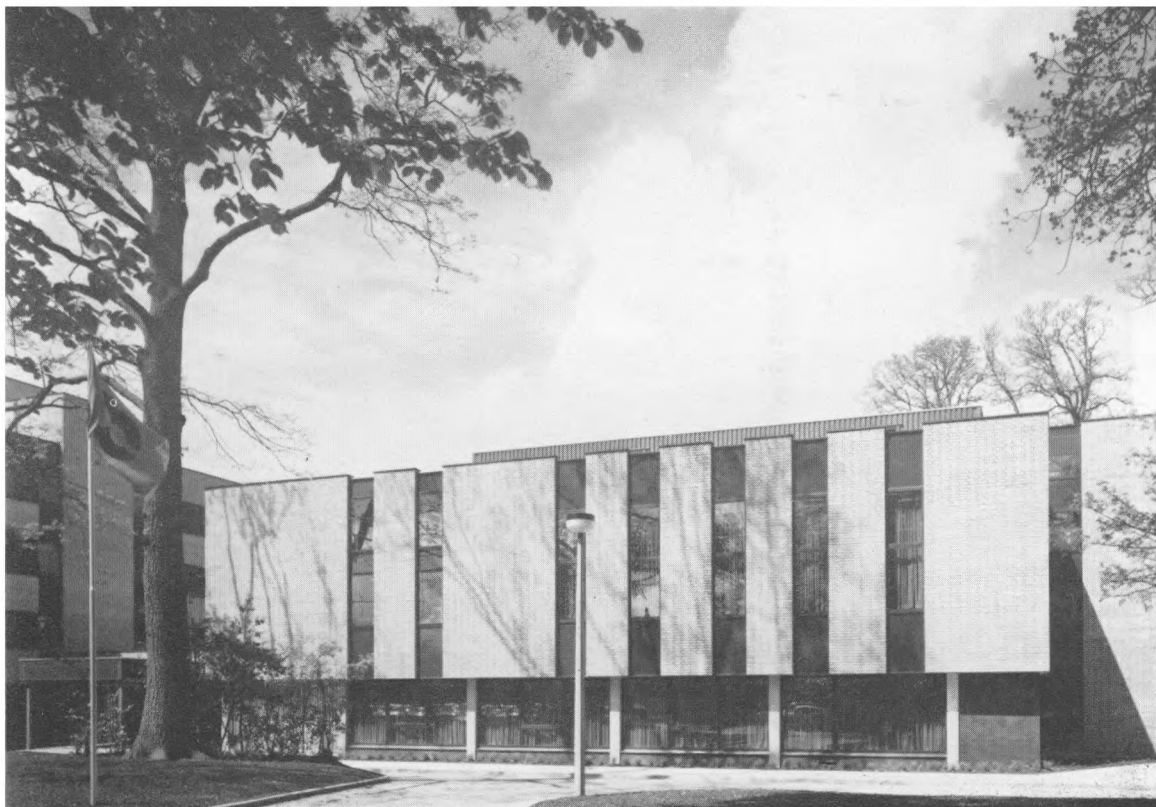


*Photograph by the Press Association Ltd.*

Plate III. The unveiling ceremony at the official opening of the European Centre for Medium Range Weather Forecasts on 15 June 1979. From left to right: the Director of the Centre (Dr A. C. Wiin-Nielsen), His Royal Highness the Prince of Wales, K.G., K.T., P.C., G.C.B., and the President of the Council of the ECMWF, (Professor L. A. Vuorela). (See page 64.)



*To face page 49*



*Photograph by the Department of the Environment*

Plate IV. The European Centre for Medium Range Weather Forecasts, Shinfield Park, near Reading, Berkshire (see page 64).

Data time 00 GMT 11 February 1979

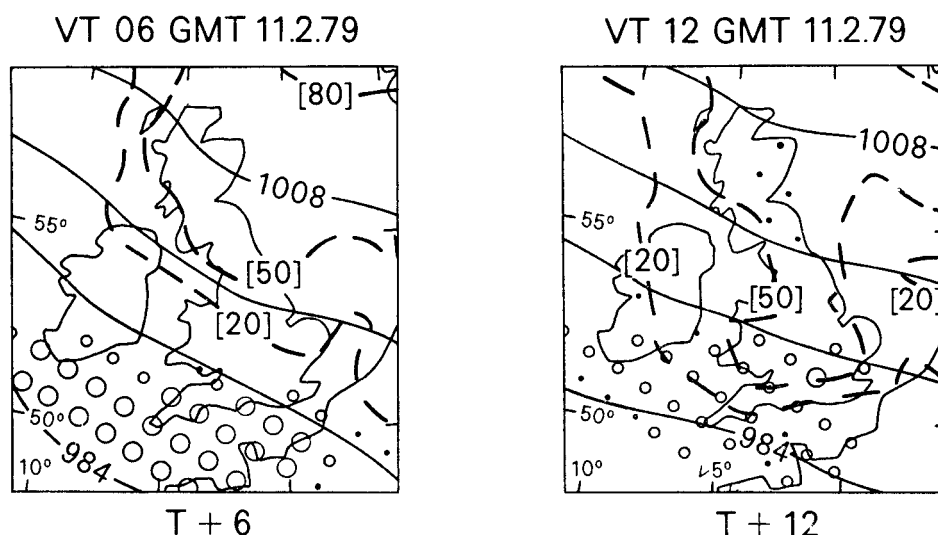


Figure 1. Example of the forecast cooling due to precipitation. Frontal rain (some dynamic rain forecast):  
 ○, ○, • = total rainfall rate > 0.5, 0.1, 0.01 mm/h.  
 Showers (no significant dynamic rain): ▽, ▽, ▽ = local convective rainfall rate > 4, 0.5, 0.1 mm/h.  
 Pecked lines represent 20, 50 and 80 per cent probability of snow, based on 1000–850 mb thickness.  
 Continuous lines are isobars at intervals of 8 mb. VT = verification time.

This value was around 30–40 per cent, indicating that the cooling forecast by the model was about right although possibly slightly too great for moderate or heavy precipitation.

Four histograms (Figures 2(a)–(d)) give the number of times a particular forecast snow probability corresponded with the snow-line. Figure 2(a) was obtained using the current operational chart output with 20 and 80 per cent probability lines, the values of probability between these values being interpolated to the nearest 10 per cent and thus being approximate. Figures 2(b)–(d) give more reliable values of probability as a 50 per cent snow-line was also drawn. Histograms were chosen as the pictorial representation of the results to highlight the approximate nature of their assessment.

Figures 2(a) and 2(b) show different distributions although the probability lines were derived from the same values of thickness. This difference results from the fact that the actual 50 per cent probability line lies much nearer the 20 per cent line, on many occasions, than an interpolated 50 per cent value, as used in Figure 2(a), between 20 per cent and 80 per cent. Thus a fixed distance on the forecast chart was interpreted as a 10 per cent range on the charts that produced Figure 2(a), whereas the same distance on the charts that produced Figure 2(b) produced up to 30 per cent probability range, resulting in a flatter distribution.

If a correction is made to the 1000–850 mb thickness to account for the height of the 1000 mb surface (i.e. a sea-level pressure correction) the mean value of the probability of the snow-line is nearer 50 per cent (Figure 2(c)). This follows since during the week in question pressure was generally below 1000 mb and the level at which snow was reported was thus nearer the freezing level than if pressure had been

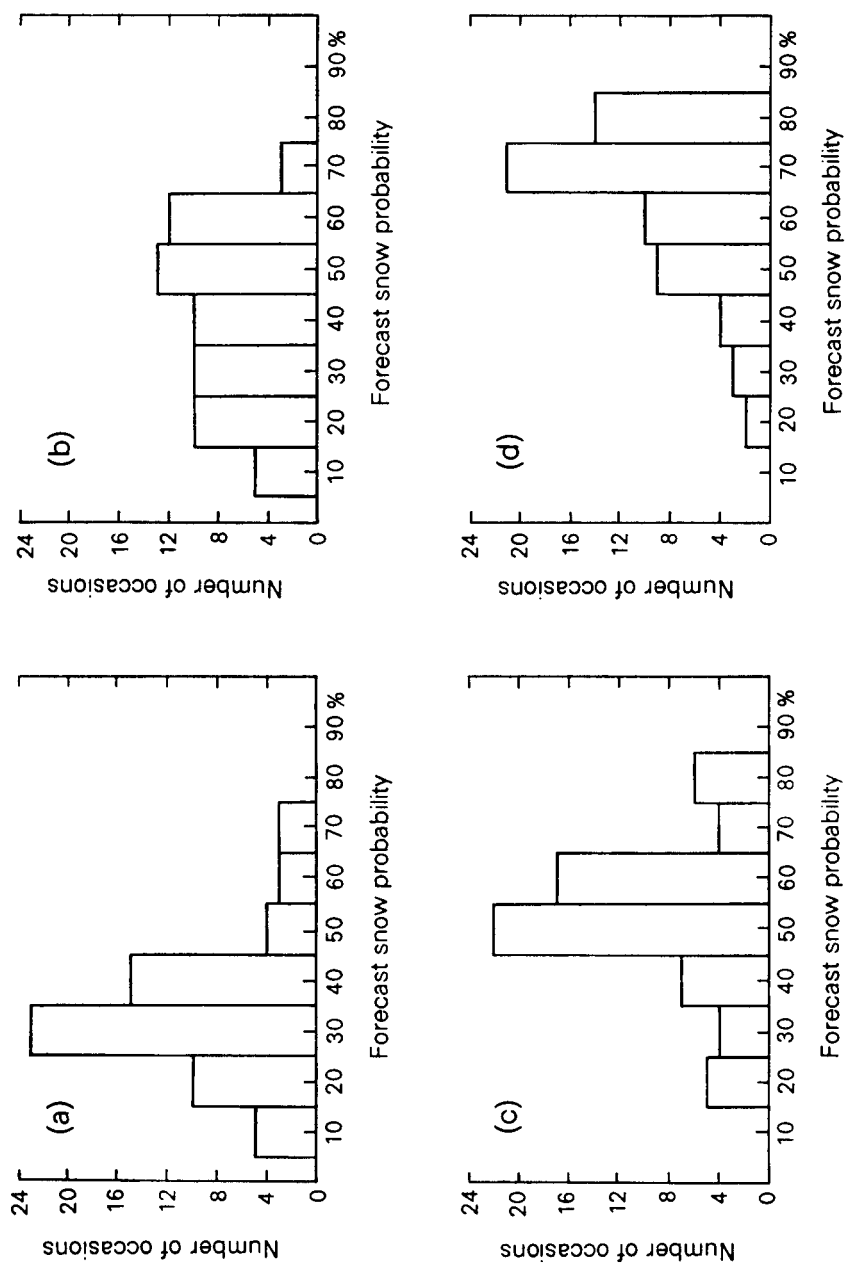


Figure 2. Number on occasions on which the snow-line coincided with various values of forecast snow probability.

(a) Probability not corrected for sea-level pressure or topographic height, and interpolated between 20 and 80 per cent isopleths.

(b) Probability not corrected for sea-level pressure or topographic height, but interpolated between 20, 50 and 80 per cent isopleths.

(c) Probability corrected for sea-level pressure and interpolated as in (b).

(d) Probability corrected for sea-level pressure and topographic height, and interpolated as in (b).



above 1000 mb. Boyden's correction adds a value to the probability to account for this. If mean sea-level pressure were above 1000 mb the mean probability of the snow-line would be expected to be lower than if pressure were below 1000 mb. So over a large number of cases, including both high and low surface pressure, a greater variability of the snow-line probability would result if no surface-pressure correction were made.

If a further correction is made for topographic height, the probability of snow increases yet again (Figure 2(d)). The mean value of the forecast probability of snow which corresponded with the snow-line for the week was then 62 per cent. One might expect this value to be near 50 per cent, i.e. half the time it would be snowing at those places on the chart where the predicted probability of snow was 50 per cent and half the time it would be raining, corresponding to the snow-line being greater than 50 per cent probability or less than 50 per cent probability. A possible reason for the value being greater than 50 per cent during the week was mentioned above with reference to the accuracy of precipitation forecasts. In this particular week there was a tendency for too large an area of precipitation to be forecast on occasions, hence giving too low a thickness or too high a snow probability. It also appears that the forecast cooling due to the precipitation may have been too great for moderate or heavy precipitation.

Interpretation of Figure 2 in terms of making a forecast is somewhat difficult since part of the range of probabilities over which the snow-line varies is due to forecast errors and part is due to the inconsistency of the 1000–850 mb snow predictor.

However, it does indicate the usefulness of this particular method of snow prediction as used in the 10-level model. Forecasters build up an experience of interpreting the forecast probabilities in terms of where snow is likely to occur. Thus a particular value of forecast probability may be used to indicate a boundary of snow with rain. Figure 2(a) was obtained from the operationally produced forecast charts of the week in question and suggests a value of 30 per cent forecast probability that the forecaster may use as guidance for the boundary. By suitable wording of the forecast indicating that snow is more likely in certain areas (e.g. over high ground or in the north) and before or after a certain time, or both, forecasts of snow can have a high success rate.

### **Some examples of forecast chart output**

Two examples of actual weather and forecasts for the same time are given in Figures 3–6. Figure 3 gives the weather at 00 and 09 GMT as actually reported on 12 February. At 00 GMT it can be seen that precipitation has broken out over a large part of England with the snow-line lying through the south Midlands into East Anglia, the snow occurring to the north of it. As a point of interest, almost all forms of precipitation are being reported at this time: freezing rain, drizzle, hail, sleet, snow, ice pellets, snow grains and rain, showing the difficulty of giving accurate forecasts of precipitation; the hail and ice pellets were taken as snow for this investigation.

The two forecasts for this time (Figure 4) have predicted the area of precipitation fairly well, although the intensity is too light in most places. Without any corrections for sea-level pressure or topographic height the snow-line is seen to be between 20 per cent and an interpolated 40 per cent with a slight increase in this probability by 06 GMT, probably due to cooling by latent heat, i.e. moving the snow-line southwards. Figure 5 shows the same surface pressure and precipitation forecasts but with a 50 per cent probability line added and also with the thickness corrections applied, as in Equation (1). The main effect of these corrections is to increase the probabilities, as would be expected.

In both cases a similar increase in probability is forecast between 00 GMT and 06 GMT, as in Figure 4, suggesting that the snow-line will move southwards. The actual weather for 06 GMT showed that the snow-line had indeed moved south, with the rain over southern England having turned to sleet

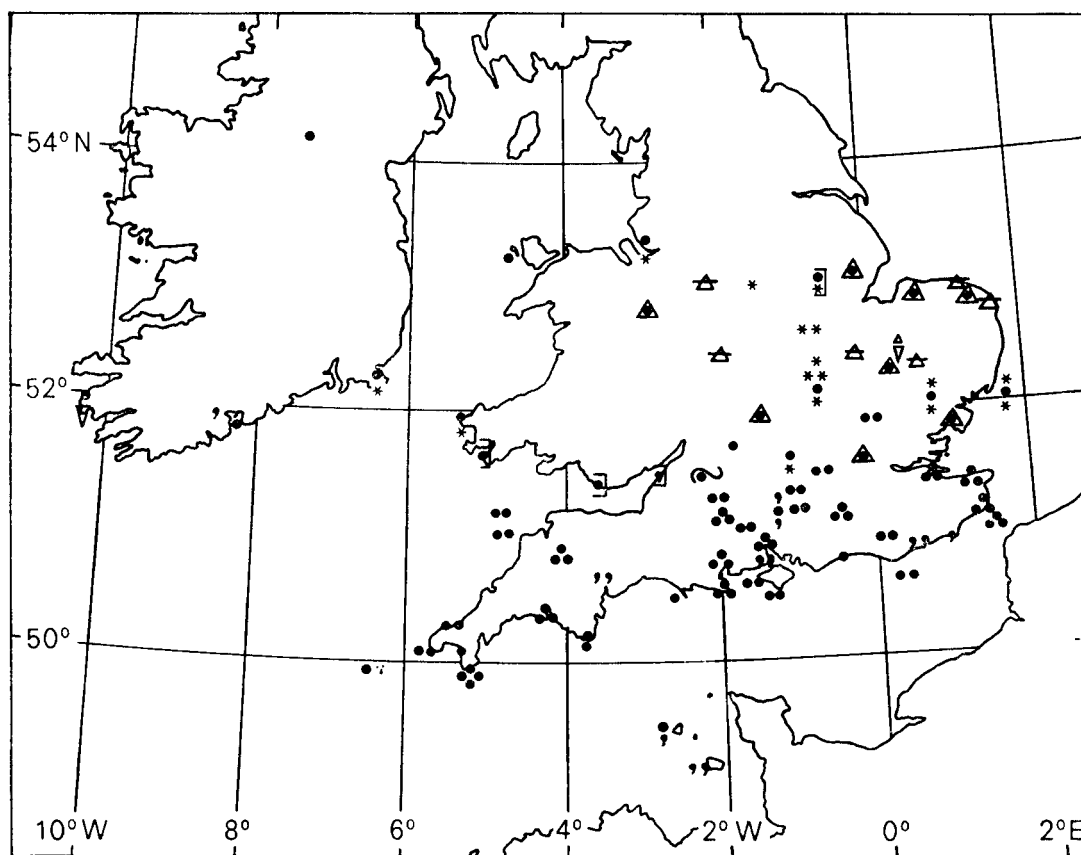


Figure 3(a). Weather at 00 GMT on 12 February 1979 over England, Wales and Ireland. The meanings of the conventional 'present-weather' symbols are given in the *Observer's Handbook*, third edition, London, HMSO, 1969.

or snow in many places. By 09 GMT (Figure 3(b)) the observations show that only a few of the stations reporting precipitation, mainly near coasts, are not reporting snow.

The second example is for 24 hours later (Figure 6); the two sets of operational forecast charts show the forecast snow-line (taken as its most frequent value of 30–40 per cent from Figure 2(a)) to have moved north again into North Wales and northern England. Observations for this time showed rain to be falling over southern England, with sleet over high ground, and snow in the north.

This snow in the north turned to rain in many places by 06 GMT except over high ground; this is indicated on the  $T + 18$  forecast by a decrease in probability of snow.

The change from snow over much of England on 12 February to rain on the 13th could therefore be predicted quite well using the forecast snow probabilities produced by the rectangle. In fact, by using the 30–40 per cent forecast probability as a snow-line, the kind of sequence shown in Figures 3–6 could be forecast for much of the week.

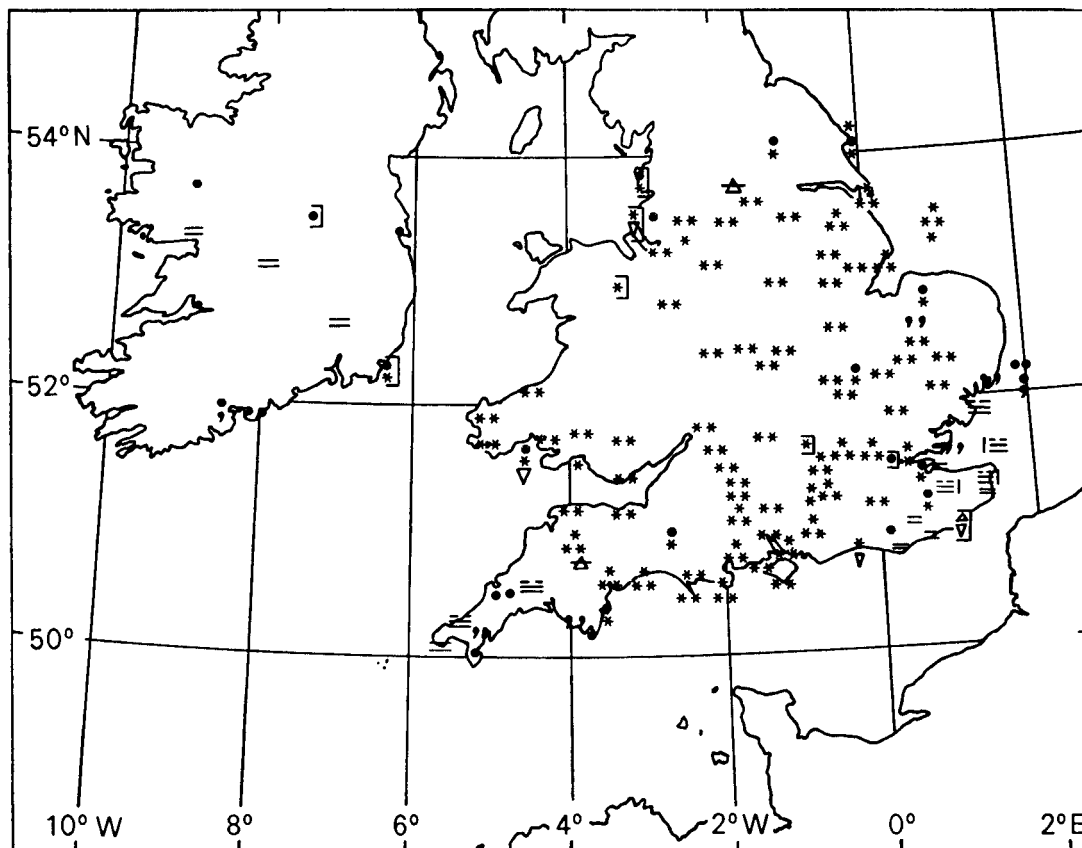


Figure 3(b). Weather at 09 GMT on 12 February 1979 over England, Wales and Ireland.

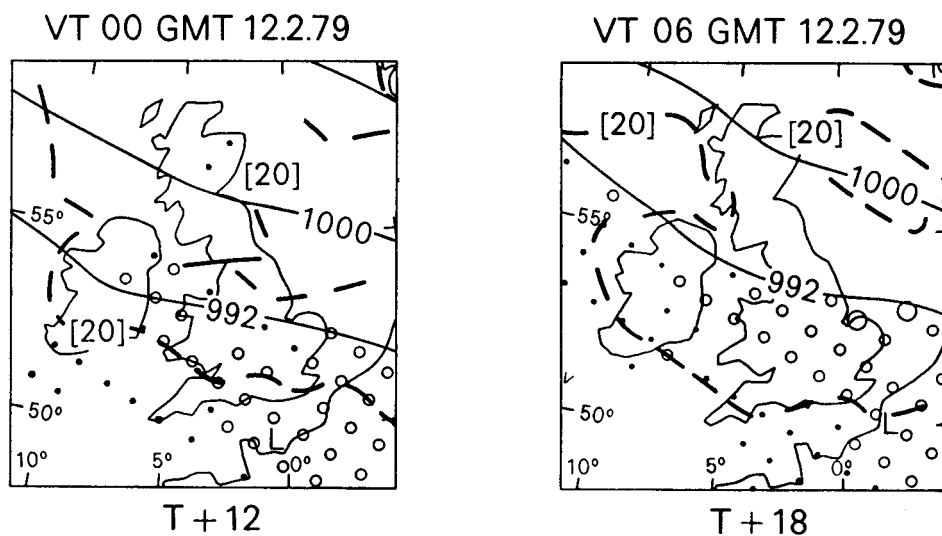
## Conclusions

During the period 9–15 February 1979 the 10-level model (fine-mesh version) gave quite a consistent indication, from forecast 1000–850 mb thicknesses interpreted as probabilities of snow together with forecast precipitation areas, of where snow and rain would occur.

Snow occurred more often than rain where probabilities of snow were forecast to be greater than 30–40 per cent using Boyden's uncorrected values of 1000–850 mb thickness. By adding an extra 50 per cent probability line to this operational output the behaviour of the forecast model could be interpreted in greater detail: for example, the cooling effect by latent heat of evaporation and melting snow on the thickness of a layer could be discerned more easily.

If corrections were made to the forecast snow probabilities to account for variation of surface height and sea-level pressure, snow occurred more often than rain at forecast probabilities greater than 50 per cent when the pressure correction was applied, and greater than about 60 per cent when both corrections were applied.

Data time 12 GMT 11 February 1979



Data time 00 GMT 11 February 1979

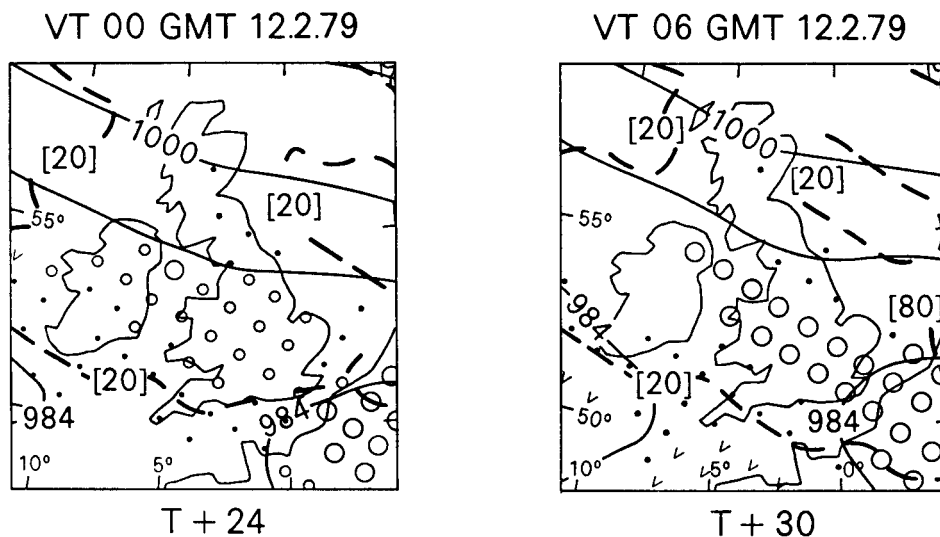
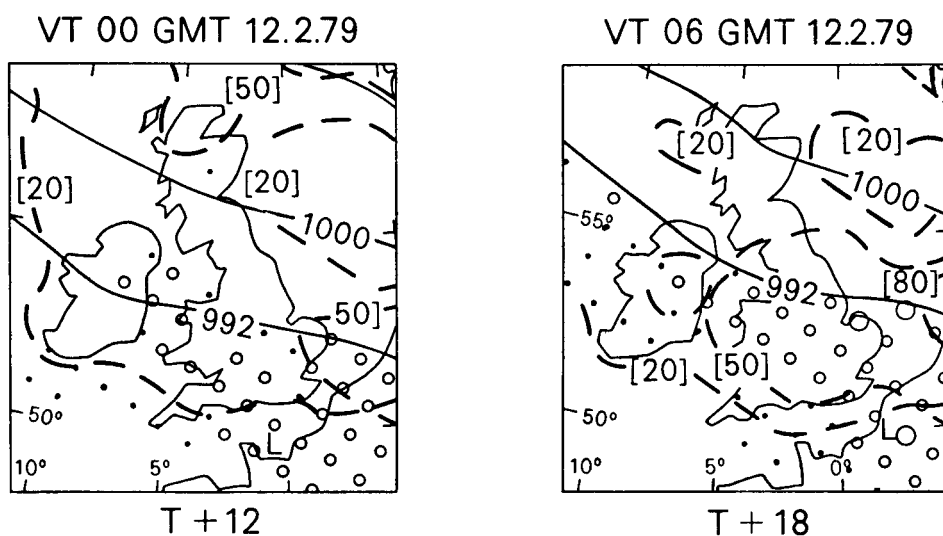


Figure 4. Operationally produced forecasts for 12 February 1979. Symbols and continuous lines as in Figure 1. Pecked lines represent 20 and 80 per cent probability of snow, based on 1000–850 mb thickness.

Data time 12 GMT 11 February 1979



Data time 00 GMT 11 February 1979

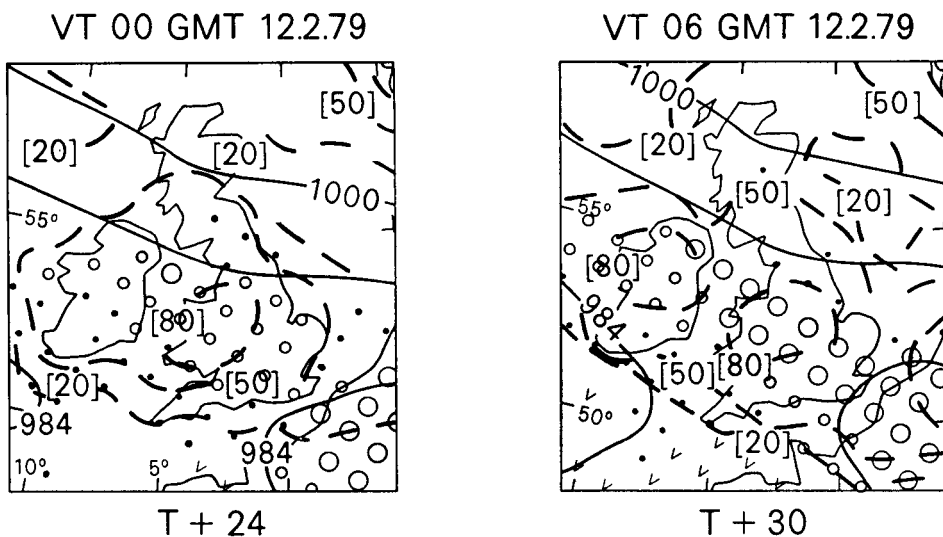
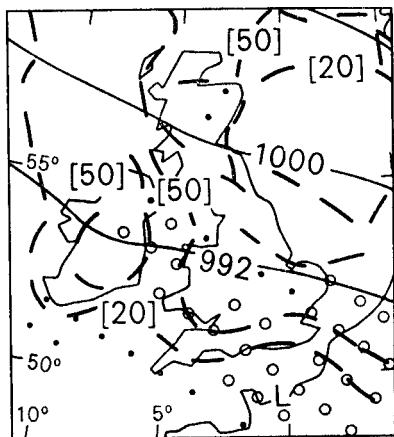


Figure 5(a). Forecasts produced using probabilities of snow corrected for sea-level pressure. Symbols and pecked lines as in Figure 1.

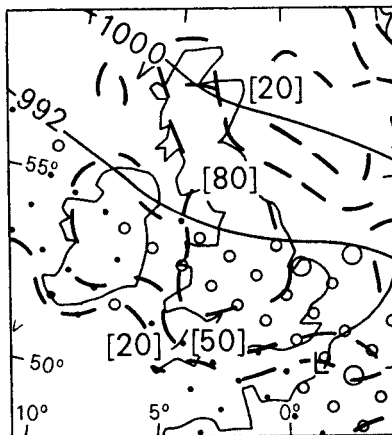
Data time 12 GMT 11 February 1979

VT 00 GMT 12.2.79



T + 12

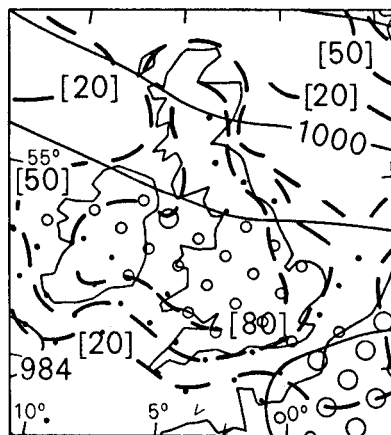
VT 06 GMT 12.2.79



T + 18

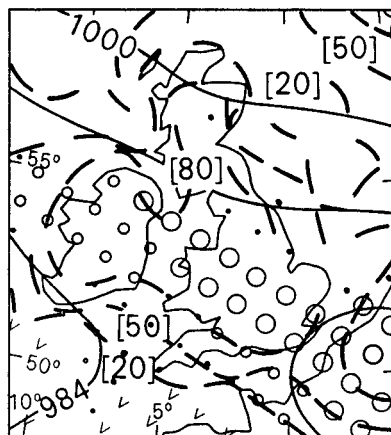
Data time 00 GMT 11 February 1979

VT 00 GMT 12.2.79



T + 24

VT 06 GMT 12.2.79

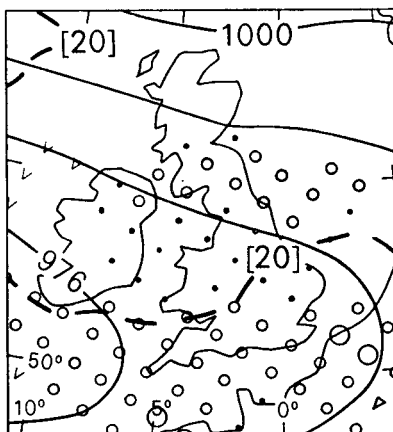


T + 30

Figure 5(b). Forecasts produced using probabilities of snow corrected for sea-level pressure and topographic height. Symbols and pecked lines as in Figure 1.

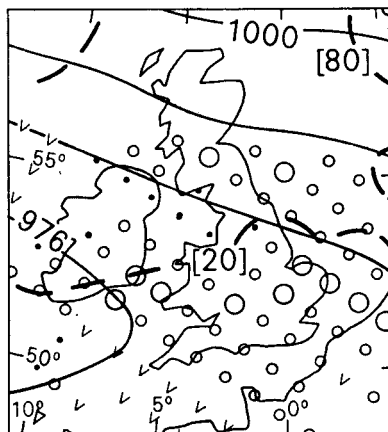
Data time 12 GMT 12 February 1979

VT 00 GMT 13.2.79



T + 12

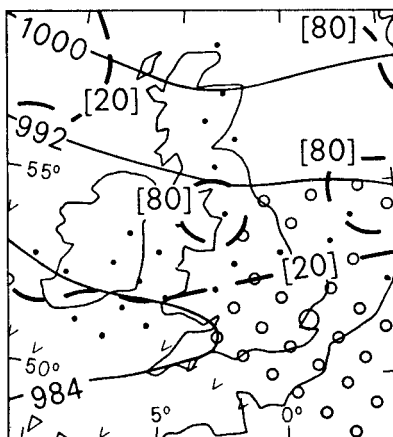
VT 06 GMT 13.2.79



T + 18

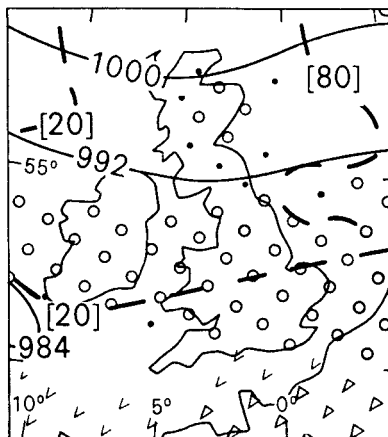
Data time 00 GMT 12 February 1979

VT 00 GMT 13.2.79



T + 24

VT 06 GMT 13.2.79



T + 30

Figure 6. Operationally produced forecasts for 13 February 1979. Symbols as in Figure 1. Pecked lines as in Figure 4.

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551.5:358:92

## Memoirs of an Army Meteorologist

By H. Cotton, M.B.E., D.Sc.

### Part 5

[Dr Cotton described the battle of Passchendaele which took place in late summer and autumn of 1917.]

The assessment of any great event should be made, not on the short term, but on its long-term consequences. In the following spring the Germans launched two offensives which were intended to end the war in their favour. The first was the drive towards Amiens to separate the British from the French. The second was to reach the Channel coast and thereby isolate the British Army from England. Both came within a short distance of success, and both failed for the same reason, shortage of men. The reserves who would have turned the scale in Germany's favour were buried in the mud of Passchendaele.

With the end of the fighting and the advent of winter the British troops near the coast were withdrawn and the extreme left wing of the Allied armies again occupied by the Belgians. Apart from the fighting in the initial stages of the war, this Belgian sector had been very quiet; in fact, aerial photographs showed the tracks made by pedestrians leading from the Belgian to the German positions. There had clearly been much fraternization. When the British took over this fraternization came to a sudden end.

A seaside place, so attractive in summer, becomes less inviting when the weather is cold and wet and the days short. Meteor left along with the rest of the Fourth Army Headquarters Staff and I went to Meteor G.H.Q. which had moved from Hesdin to Montreuil. Montreuil is a walled town of great historical interest. It stands very high and the encircling wall is complete, a walk round it being a pleasant experience and affording magnificent views of the countryside. It was known to Henry V, Shakespeare's King Hal, 'with Agincourt for glory and the stake for zeal', who passed through it on the way to Paris. He passed through it again but in the opposite direction, a corpse on the way to burial in England.

The work at Meteor G.H.Q. was very similar to that at any forecasting establishment, even in peace time. There was the difference that a hopelessly wrong forecast, and a military operation based on it, might have disastrous results. As far as I remember there was no forecast so far out that it led to serious consequences. The chief task of the day was the preparation of the synoptic chart for the fundamental time of 7 a.m. since the forecast was based on this. This chart, along with the current situations at the various observer stations, was taken to the Colonel as soon as it was prepared. He then made his forecast and the chart, weather particulars from the forward stations and the current forecast were drawn in copying ink on a general weather chart. Copies were then made on a jellygraph—modern copying machines were not then invented—and these were distributed to the various headquarters at G.H.Q. Early in the forenoon the Colonel visited the Commander in Chief and discussed with him the



weather prospects for the day. Reports from local stations arrived throughout the day and at 6 p.m. the data for a new synoptic chart were received and the process repeated except that a general weather chart was not prepared. I appreciated the very great importance of the work at G.H.Q. but somehow I missed the excitement of the nearness to the fighting lines, particularly at the single observer stations like Le Touret.

Early in the new year of 1918 I was commissioned 'in the field' and became a Second Lieutenant, R.E. As far as the work at Meteor was concerned it made very little difference. A great change was the officers' mess which was managed on the lines of a good hotel. The waitresses all belonged to the W.A.A.C. and ours, at the Meteor table, was a very pretty girl named Edith. I never knew her other name. The food was a far cry from bully-beef and dog biscuit stew or, when things were bad, just dog biscuits without the bully-beef or the stew. The disadvantage of being commissioned was, from my point of view, the almost continuous saluting for it now included all non-commissioned ranks as well as the commissioned ranks from major upwards. Also, as it was G.H.Q. the powers that be were very snooty over such things. Occasionally I would visit one of the forward stations, to take a new member of Meteor or sometimes a replacement for a man who was going on leave. As the journey was made by G.H.Q. car I looked forward to such jaunts since it was a good way to see the country. It was amusing to see everybody stiffen up when they noticed the blue and red flash of G.H.Q. and I am sure that there were many unkind thoughts when men of much higher rank than mine realized that they had saluted a mere second lieutenant.

About the middle of February I went to Third Army Headquarters at Albert as a Meteorological Officer, and I felt that I was getting back into the heart of things. I looked at the famous hanging madonna and wondered about the legend. The Meteor office was in a very pleasant house some distance from the town on the Bapaume road. One day, walking along the road I came across the wayside grave of Colonel Wedgwood of the Fifth North Staffs. As I had met his wife previously I made a sketch of it and sent it to her. As the great German spring offensive against the Third and Fifth Armies started almost immediately after I posted it, it is possible that she never received it.

During the month of March 1918 the weather was established anticyclonic with light easterly winds, a clear sky both day and night and bitterly cold nights. A request was made for a gas attack on a troublesome sector of the Hindenburg defence system in the neighbourhood of the village of Noreuil. On the face of it such a request at a time of such meteorological conditions seemed sheer lunacy, but there were two factors which were favourable. The first was the new method of delivering the gas. The original method of releasing chlorine gas from cylinders was abandoned in favour of delivering the gas straight on the target by firing canisters of the gas from trench mortars. These did not explode but opened gently so that the gas was in the greatest possible concentration.

The second factor was the nature of the terrain. The Germans were masters in the design of defensive systems and all the way from the sea to the Swiss frontier they held the high ground. But in the neighbourhood of Noreuil there was a system of ridges and valleys, almost like the fingers of the hand, running in a roughly west-east direction. Even the Germans could not site their defences along contour lines in such a terrain and consequently they had to dip down into the valleys. This favoured the exploitation of a katabatic wind in the early hours of the morning, the best possible time for the maximum psychological effect of such an attack. Was this possible? It was the task of Meteor Third Army to find out.

The first thing was a request to Meteor G.H.Q. for a forecast covering as many days as possible and, because of the great stability of the anticyclonic system and no sign of its being displaced by a cyclonic system we were assured of stable conditions for, at least, several days. Continuous observations for two whole days and nights were therefore made of the winds in these valleys and it was found that the

gradient wind ceased well before midnight and then there was a gradual build up of the katabatic wind reaching about three miles an hour, an ideal velocity for such an attack, at about 3 a.m. The go-ahead for the attack was then given and in the early morning hours tons of pure phosgene gas were delivered from trench mortars. Phosgene gas, besides being a deadly poison, is extremely heavy and therefore, when gently released from the canisters, instead of a violent dispersal, it poured almost like a liquid into the trenches and dug-outs. I do not know whether gas masks offered any protection against this gas provided there was air to breathe, but there *was* no air to breathe. The phosgene in sinking simply displaced the air so that the gas masks, however efficacious, were useless. The casualties were very heavy.

It was a foul way to kill fellow human beings but the Germans started it. An atrocious weapon is only justified if by its use the war will be almost immediately ended. If it is not, then the other side will develop something even more atrocious, and so the Germans paid heavily for using gas in the first place. Without the co-operation of the meteorological services, both at Army Headquarters and at G.H.Q., this gas attack would not have been possible. It was the last gas attack of the war, but gas shells fired from guns were used right up to the end of the war.

The Germans used phosgene gas in the later stages of the Verdun battles and they believed that, at last, they would achieve a breakthrough and capture Verdun. They were so confident that they actually brought up military bands to head the triumphant march into the town. The gas was delivered in shells by the artillery, not in canisters from trench mortars as in our case, although the effect was at first the same. Because of the element of surprise and of a defect in the fabric of the French gas masks, which were not impervious to the gas, an advance greater than any since the very beginning of the Verdun battles was made. After that the attack petered out for two reasons, the first being that the French troops were hurriedly equipped with masks which were impervious to the gas. The second was the nature of the terrain. Practically every square foot of land in the battle areas had been ploughed up time and time again by the artillery of both sides. The French had no chance to construct the German type *Stollen*; in fact it would have been impossible to do so, the best they could achieve being hastily dug trenches. Consequently the gas, although it settled in hollows, dispersed horizontally and was thereby considerably diluted. In the Hindenburg line the deep dug-outs, although an almost perfect protection against artillery, were death traps in the case of an attack with a heavy lethal gas like phosgene because there was no possibility of dilution by dispersion. The atmosphere in the dug-outs became one of phosgene, not air, and so gas masks, no matter how efficient, were useless. I was told that we used, along with the phosgene, canisters of a penetrating gas which made the men remove their masks. It would have made no difference to those in the deep dug-outs but would have been effective in the case of those nearer the surface.

[Dr Cotton then describes the German attack on the British Third and Fifth Armies which took place in March 1918.]

What happened to the Fifth Army affected the Third Army and therefore affected me. Without doubt the southern half of this Army would have been compelled to fall back, apart from what happened to the Fifth, for with a bombardment of that nature, combined with the erroneous troop dispositions and the inadequacy of the defences, a stand was quite impossible. In addition it was necessary for the right flank to keep pace with the retreating Fifth Army left flank. The whole Army therefore pivoted on its junction with the First Army to the north.

I doubt if anyone in the Third Army Headquarters went to sleep that night. There was a very thick fog before the barrage started at 5 o'clock and immediately after breakfast I set out to bring in the forward observers. The whole of the Meteor equipment and records were then loaded on to a lorry and we, along with the rest of the Headquarters Staff, became the precursors of a general retreat. As we

passed the basilica I looked up at the hanging Virgin. She was still in her precarious position but very soon after she was brought down. It looked as though the prophecy was about to be fulfilled but with a German instead of an Allied victory.

As we retreated, French people came to the doors of the houses to watch the British going the wrong way. It was distressing to see the looks of despair on their faces although, as it happened, the German advance was halted at Albert.

The new Third Army Headquarters was at two neighbouring villages, Beauquesne and Terramesnil. They were pleasant, the countryside was pleasant, the sky was blue and the wind warm, and there was a carpet of anemones in the woods. Almost immediately I had to go again to the kite balloon, this time for the duration of the war.

It was very soon realized that the same balloon could not be used in a dual role, for meteorological observations and for artillery spotting, the main reasons being that meteorological ascents had to be made at fundamental hours in order that they could be correlated with ground-level observations, also that they had to be made no matter what the weather was like provided it was not such as to endanger the balloon as, for example, very high winds. So ascents were made under conditions which grounded balloons engaged solely in artillery work, very low cloud cover for example, or almost nil visibility.

So when I arrived the balloon had already been taken out of the line to the village of Strazeele about four miles west of Bailleul. Like practically all the villages in north-east France it was drab; in fact, I had the impression there must be a reason for this universal drabness. Perhaps past history provides the answer. The land on either side of the Franco-Belgian border is not called the cockpit of Europe for nothing.

The balloon personnel were not those I had worked with before; the two officers were Lieutenant West and Lieutenant Donkin, always referred to as Dinkle, although I never knew why. He was a good balloon man but all he could think of was girls, and it was clear that he had already found the village decidedly frustrating. He was only nineteen.

A few days after my arrival I was making the 7 a.m. ascent with Dinkle in charge of the balloon. He was in a bad temper, possibly because I had ordered the batman to pour cold water on his head as the only means of wakening him. A particularly loud alarm clock held right against his ear produced no effect whatever apart from a slight shuffling. An alternative method was to tip him out of bed but he would have continued to sleep on the floor if left alone. So he took a poor view of my joke about 'let go the guys'.

On 19 April 1918 the Germans commenced the second of their spring offensives and I thought I must be a kind of Jonah, being followed around by German offensives everywhere I went. On the first day they attacked on a front of 11 miles, extending this to 24 miles the next day. As the Germans had shown at Verdun, kite balloons can move forward during an advance and thus act as the eyes of the artillery but during a retreat they have to be moved to safety as quickly as possible. Early on the 10th we began to evacuate Strazeele. This meant deflating the balloon and packing it up, loading everything on to lorries, including a large number of gas cylinders, not to mention a vast amount of smaller stuff. I had seen to the disposal of my instruments in the light tender and that was all I could do apart from trying not to be in anybody's way. When the cavalcade was about to move off I noticed Lieutenant West approaching me in a great hurry.

'Will you do something for me?' he said.

'Yes, of course, if I can.'

'Wing has just rung through to say that a ferry pilot has ditched a brand-new plane with a secret engine in a field next to the Lunatic Asylum east of Bailleul, right in the way of the German advance.'

As we are the nearest to it they want it sabotaged. But I can't spare Dinkle as we must be out of here as soon as possible, so will you do it?"

'Good gracious,' I said, 'I haven't the foggiest idea how to sabotage anything, let alone an aeroplane. Besides, my O.C. wouldn't like my taking on a thing like this without his consent. I must ring Meteor.'

'You won't get through.'

We tried and he was right. Eventually I said that I would have a go. Apparently all I had to do was to pour petrol over everything and set it alight. It sounded too simple. The R.F.C. were supplied with P and M motor-bikes and sidecars, the bikes having 500 c.c. single-cylinder engines with the cylinder inclined so as to be parallel to the front member of the frame. I had never driven one but my batman was familiar with ours. So I told him to drive and I sat in the sidecar, and we stowed sufficient petrol in cans to set fire to the whole Air Force. Once again there were the pitiful streams of refugees burdened with as much of their belongings as they could take away. It was almost impossible to make any headway, and as I had no idea how fast our own army was retreating the delay caused considerable anxiety. When we reached Bailleul, the town seemed to be empty and it was being shelled, although not very heavily. It was hop-growing country and I noticed several fields almost filled with the tall poles.

We reached the field and there was the plane, and in the next field was the Asylum, a red-brick monstrosity.

'That's where we ought to be', my batman said and I was inclined to agree with him. Just before we started pouring petrol over the plane, I had an idea.

'How are we going to set it alight?' I said to the batman.

'Bloody hell,' he said, 'I never thought of that. We shall very likely go up with it.'

I thought of the long hop poles and sent him to bring one. I tied a bundle of rag at the thick end so that it would travel true when thrown as a spear. Then we poured the petrol over what I thought would be the most important parts—there was quite a lake of petrol under the plane and this would help. I then poured petrol over the rag, set fire to it, threw the makeshift torch at the plane and then threw myself face down. There was a terrific whoosh and a hot blast like the inferno. The plane would certainly be a write-off after that although, of course, I had no idea whether the secrets of the engine had been destroyed. When we reached Bailleul on the way back the town was being heavily shelled and as we were crossing the end of the *Grand' Place* a shell made a direct hit on the clock tower of the *mairie* and I saw the beautiful blue clock tumbling down.

We had made only 20 or 30 yards further on when the bike stopped. An examination showed that the float was punctured. 'Now what do we do?' moaned my driver. I remembered an article I had read in the journal *Motor Cycling*. The writer had substituted a large cork for the punctured float. We were fortunate. A little further on there was a chemist's shop. The door was locked, of course a useless precaution since it was certain the Germans would soon capture the town and loot everything. We broke in by smashing one of the windows with our spare petrol tin and, while my batman was looking round for what he could scrounge, I looked for corks which I found in a many-drawer cabinet. I took several, of different sizes, and fortunately one of them was just right. The repair was facilitated by the fact that the carburettor was a top-feed AMAL. If it had been a bottom-feed B and B—these were the two makes in most common use in those days—the makeshift repair would have been more difficult. Strangely enough I had to make a similar repair to a Blackburn machine I bought soon after demobilization and the cork was still functioning as the float when I sold it.

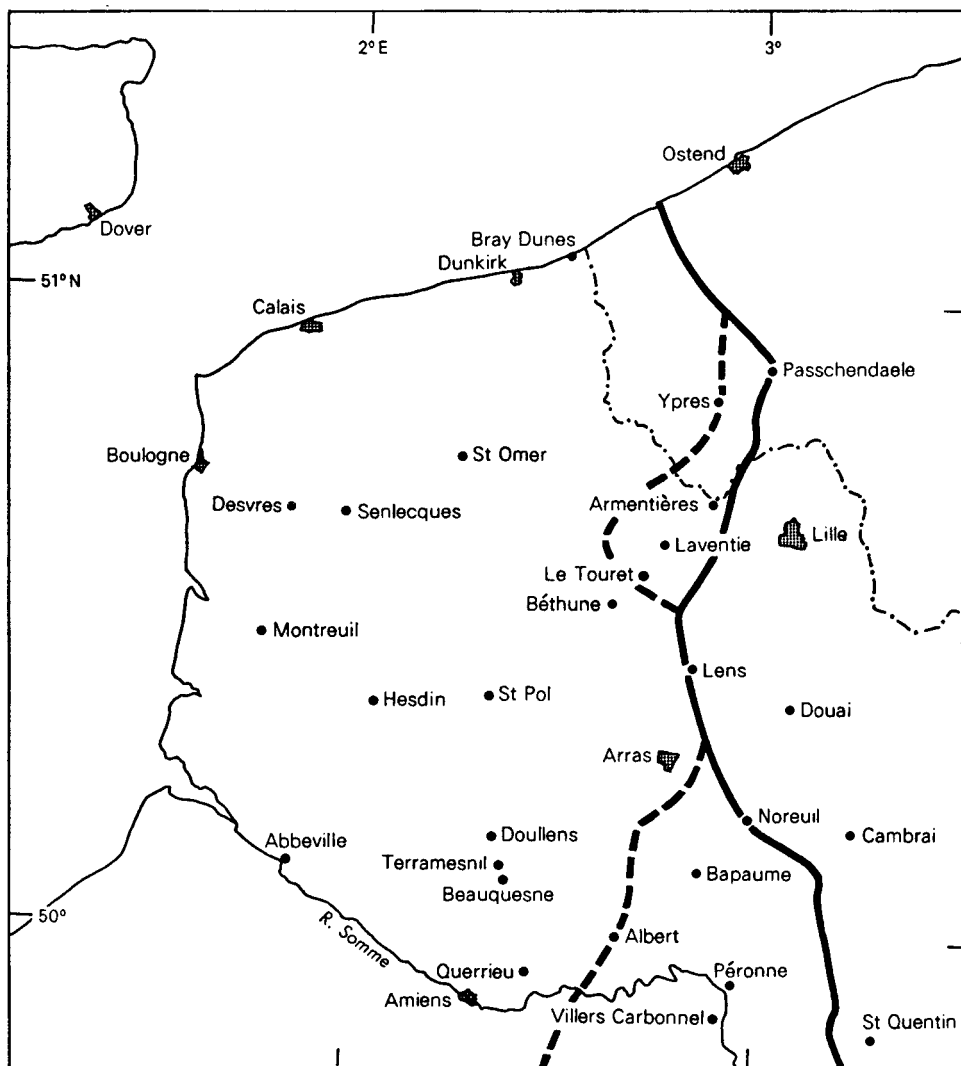
It was dark when we reached the new camp. I forget the name of the village, which was very small. I found Lieutenant West and gave him an account of all that we had done, and told him that if he didn't recommend me for the V.C. he ought to be shot at dawn. As it was, I thought it best not to court trouble by informing G.H.Q. so I heard nothing more about it.

The balloon was reinflated and the routine of three ascents a day was restarted as though nothing had happened.

The aim of this second German push was to reach the coast and thereby end the war 'at a stroke'. Fortunately they failed once again and their bolt was shot for good. I heard that they had overrun Le Touret, and that is as far as they got in that sector.

*(To be continued)*

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Map showing towns in Flanders and the Pas de Calais together with villages mentioned by Dr Cotton. The solid black line shows the rough position of the front line at the beginning of 1918, and the dashed line the extreme position reached by the German advance in the spring of that year. The Franco-Belgian frontier is also indicated.

551.5:06

### **Visit of His Royal Highness the Prince of Wales to the Meteorological Office, 15 June 1979**

On the morning of 15 June 1979 His Royal Highness the Prince of Wales made an informal visit to the Meteorological Office. On arrival His Royal Highness was greeted by the Lord Lieutenant of Berkshire, Lieutenant Colonel the Honourable G. W. N. Palmer, who presented Sir John Mason, the Director-General. Sir John presented to His Royal Highness Dr K. H. Stewart, the Director of Research, Mr F. H. Bushby, the Director of Services, and members of the Senior Directorate.

Sir John conducted His Royal Highness to the Central Forecasting Office (CFO) where Mr D. E. Jones, the Assistant Director in charge, explained the work of CFO and presented several members of the staff who explained their work to His Royal Highness.

His Royal Highness also visited the COSMOS Computing Installation where the Director-General presented Mr W. R. Brady, the Computer Manager, who described the Installation and the work performed.

On his departure from the Office, His Royal Highness signed a specially illuminated page of the Visitors' Book. (See Plates I and II.)

551.5:06

### **The official opening ceremony of the European Centre for Medium Range Weather Forecasts**

The official opening ceremony of the permanent Headquarters buildings of the European Centre for Medium Range Weather Forecasts took place on 15 June 1979. The opening ceremony was performed by His Royal Highness the Prince of Wales.

Following his visit to the Meteorological Office in Bracknell on the morning of 15 June 1979, His Royal Highness the Prince of Wales arrived at the European Centre for Medium Range Weather Forecasts in Shinfield Park at 11.20 a.m. He was accompanied by the Lord Lieutenant of Berkshire (Lieutenant Colonel the Honourable G. W. N. Palmer), Chief Inspector J. Maclean, and his Assistant Private Secretary (Mr O. Everett). The President of the Council of the European Centre for Medium Range Weather Forecasts (Professor L. A. Vuorela), the Vice-President of the Council (Mr R. Mittner), and the Director of the Centre (Dr A. C. Wiin-Nielsen) were presented to His Royal Highness.

The President of the Council conducted His Royal Highness to the reception area in the Centre and invited His Royal Highness to unveil the plaque commemorating the visit. The wording on the plaque read:

'This building was opened by H.R.H. the Prince of Wales, K.G., K.T., P.C., G.C.B., and presented on behalf of Her Majesty's Government to the President of the Council, Prof. L. A. Vuorela on behalf of the European Centre for Medium Range Weather Forecasts on 15th June 1979.'

After the unveiling, representatives of the Property Services Agency of the Department of the Environment and John Laing Construction Ltd, the building contractors, were presented to His Royal Highness. The Director of the Centre, accompanied by the President and Vice-President of the Council, then escorted His Royal Highness into the Computer Hall, where the party first visited the Meteorological Operations Room. The Director of the Centre gave a description of how medium range forecasts were to be produced operationally. Some of the Centre's current medium range forecasts were

then presented, including forecasts verifying on 15 June and predictions for the following week. The party then entered the Computer Room where His Royal Highness was given explanations of the functions of the various parts of the computer installation.

Upon completion of the tour, the party returned to the reception area, where His Royal Highness was invited to sign the Visitors' Book. After the signing the Director led His Royal Highness and the rest of the party to the marquee erected specially for the occasion in the grounds of the Meteorological Office College at Shinfield Park, immediately adjacent to the site of the Centre buildings. There, in front of the assembled staff of the Centre and the visitors, the Director of the Centre formally welcomed His Royal Highness. In reply His Royal Highness spoke of the complexity of the systems which went into the difficult task of weather forecasting, but emphasized the major economic and social advantages that were obtained by successful predictions. The President of the Council then thanked His Royal Highness for the presentation on behalf of Her Majesty's Government of the Centre buildings, and for formally opening them. In conclusion Professor Vuorela presented His Royal Highness, on behalf of the Centre, with a cut-glass decanter. A cheque for £1000 was also sent to the Prince's Trust.

Following the speeches in the marquee, the Director of the Centre escorted His Royal Highness and the rest of the party to the car park at the Centre. The Director of the Centre, the Vice-President of the Council and the Lord Lieutenant of the County took leave of His Royal Highness, who departed from the Centre at 12.30 p.m.

(See Plates III and IV.)

## Reviews

*The physical environment*, by B. K. Ridley. 235 mm × 145 mm, pp. 236, *illus.* Ellis Horwood Ltd, Publishers, Chichester, 1979. Price £8.50.

This book, according to the author's preface, is intended to 'appeal to students of science and engineering, whether physical, biological, or social', and is based on a course for first-year science and engineering undergraduates at the University of Essex. This defines the standard of difficulty and depth of treatment of the text which provides a broad survey of astronomy, geophysics and meteorology; the topics dealt with include tidal friction, seismic waves, earthquakes, continental drift, natural forces and their manifestations from the atomic nucleus to tornadoes and the gulf stream, atmospheric radiation, and the origins of the universe and of life. It is obviously not a book written for the professional meteorologist, or the professional expert in any of the various disciplines treated, but is to be judged by how well it succeeds as a book of relatively advanced 'general science'. In general plan, and in basic quality of discussion and treatment, it is indeed a good and interesting book with many excellent plates and diagrams both in colour and in monochrome (some as fold-outs), 'optional' mathematical discussions set in small type, and useful bibliographies for 'further reading' at the end of each chapter. The text would, however, have benefited greatly from a thorough revision before being printed when a number of stylistic and grammatical slips might have been picked up; for example, 'a phenomena', 'pertain' for 'obtain', 'perform an average', 'continually' for 'continuously', 'emulate' for 'paraphrase', and, in particular, the section on the gulf stream where 'southern', 'southwards', 'south' and 'southerly' follow in quick succession in a confused piece of prose which would baffle anyone in an attempt to understand in just which directions the wind was blowing and the water moving; the account of the thermal wind, too, starts in a most misleading way.

On matters of fact, it is not true to say that 'a variation by about 0.5 per cent in the amount of sunlight received by the earth has been observed this century'. Claims that such a variation had been observed were indeed made some decades ago, but it is now known that the uncertainties of observation were too large for the variation to be accepted as real. (Better data may be obtainable in the future from satellites.) The albedo of land is far from constant at 20 per cent (p. 84), and varies from 12 per cent (tropical rain-forest) to about 35 per cent (desert).

The standard of textual editing is deplorable. Misprints abound, and abbreviations of unit symbols break—at random—all the rules of the SI system. On p. 151 the universal constant 3.14159... is represented by  $\Pi$ , not  $\pi$ ; Mohorovičić has lost his diacritics; in Figure 7.12, the diagram clearly shows an italic  $l$  as a subscript indicating the component of velocity along the pressure gradient, but the mathematical equations have a curious symbol resembling a rotated script 'e'.

The problems at the end of the book contain some oddities. The first (on Olbers' paradox) is really statistical, and the solution given—which assumes that all the opaque spheres contained in a volume of radius  $R$  are uniformly distributed over the surface of that volume—can only be a probability one. In problem 7.20, it should perhaps be made clear that the weather maps reprinted from *The Times* are forecast ones, not actuals.

A pleasant feature is the selections of prose and poetry after the chapter headings; they are obviously culled from a wide range of reading—much wider than dictionaries of quotations! The author's own style, however, is often too heavily jocular for the present reviewer, as when we are told of the 'maniac' observer at the centre of a rotating frame of reference, and of the 'fooling about' of the stars and sun.

To sum up: a book excellent in conception and general plan, but marred by lack of revision and by bad editing.

R. P. W. Lewis

*Turbulent shear flows I. Selected papers from the First International Symposium on Turbulent Shear Flows, The Pennsylvania State University, University Park, Pennsylvania, U.S.A.,* edited by F. Durst, B. E. Launder, F. W. Schmidt and J. H. Whitelaw. 245 mm  $\times$  165 mm, pp. viii + 415, illus. Springer-Verlag, Berlin, Heidelberg, New York, 1979. Price DM 98, US\$53.90.

*Turbulent shear flows I* contains selected papers from the First International Symposium of that name grouped under five section headings, namely Free Flows, Wall Flows, Recirculating Flows, Developments in Reynolds Stress Closures, and New Directions in Modelling. The editors must be commended on their careful selection of material, which has produced a very readable and clear account of research presented at the Symposium. The printing and the reproduction of diagrams are also of a uniformly high standard. It must be pointed out that most of the papers, in fact all of the first three sections, are concerned with flows from engineering situations. The section on Reynolds Stress Closures contains the only three papers directly concerned with meteorological flows; these papers discuss the effects of buoyancy in the planetary boundary layer. However, the rest of the book, especially the last section which has an emphasis on subgrid modelling, contains many fundamental papers of interest to anyone studying the dynamics of turbulent shear flows. On the whole, I think that the high quality of production and the careful editing of material definitely justifies the slight delay in publication of these conference proceedings.

R. I. Sykes



## **Award**

We note with great pleasure that the twenty-fourth International Meteorological Organization Prize for outstanding work in meteorology and international collaboration has been awarded to Professor Helmut Erich Landsberg (United States of America).

Professor Landsberg has established himself as one of the world's leading figures in climatology and associated disciplines. He was one of the first to realize that climatology would need to be given a much higher priority by those responsible for dealing with world affairs—a challenge which WMO is now meeting by the recently approved World Climate Program.

## **Notes and News**

### **'Earth' and 'soil' temperatures**

For many years the Meteorological Office has described temperatures measured below ground level as 'earth' or 'soil' temperatures according to the depth of the measurements. It has now been decided that all such measurements will in future be referred to as 'soil' temperatures, in conformity with a recent World Meteorological Organization recommendation.

### **Enhancement of the Meteorological Office computer system**

An order has been placed by the Central Computer Agency with Control Data Limited, the U.K. subsidiary of the large U.S. computer company Control Data Corporation, for the supply to the Meteorological Office of a CYBER 203E computing system. This computer is an enhanced version of their existing CYBER 203 machine. It is expected that it will meet, with a comfortable margin, the minimum requirement for ten times the power of the IBM 360/195 currently used at Bracknell, Berkshire. The new computer is planned to operate in conjunction with the existing IBM computers and is scheduled for installation early in 1981.

The CYBER 203E will enable the Meteorological Office to study the atmospheric processes which determine the global climate. The large variations in the general circulation of the atmosphere, which cause big differences in regional weather patterns from year to year, and the systematic changes in climate which might be inadvertently brought about by Man's activities, are of world-wide concern and this has led to the institution of a World Climate Program, supported by nearly all nations, beginning in 1980.

The global climate will be simulated numerically by an advanced computer model and the influence of relevant factors studied by observing the response of the model to variations in these. A better understanding of the mechanisms responsible for climate should improve long-range weather predictions and enable better judgements to be made on the likelihood of systematic changes.

The computer will also be used to develop methods for producing more detailed short-period forecasts for specific areas of the British Isles and will take over the routine production of forecasts for a few days ahead.





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No. 1291

February 1980

Vol. 109

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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

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Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd, 24-28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by

HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 586891 K15 2/80

Annual subscription £20.82 including postage  
ISBN 0 11 722058 2  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

March 1980

Met.O. 931 No. 1292 Vol. 109



# THE METEOROLOGICAL MAGAZINE

No. 1292, March 1980, Vol. 109

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551.574.42:621.315.175

## **The formation of ice on electrical conductors during heavy falls of wet snow**

By A. K. Kemp

(Meteorological Office, RAF Valley)

### **Summary**

An example is described of severe disruption to electricity supplies caused by the freezing of wet snow on conductors, and an explanation is offered of the freezing mechanism. The same mechanism is used to account for the freezing of wet snow on locomotive pantographs on electrified main lines.

### **Introduction**

The snowstorm of 16/17 March 1979, which affected mainly the northern half of England and Wales, severely disrupted electricity supplies over north-west Wales. According to MANWEB (Merseyside and North Wales Electricity Board) engineers, Anglesey was by far the worst affected area with damage to high-voltage overhead equipment at approximately 70 locations. The damage on Anglesey, which occurred on the night of 16/17th, mainly consisted of the breaking of overhead conductors by ice loading but there were examples of broken poles or pole-top steelwork. MANWEB engineers reported that conductors were covered with a core of ice about 5 cm in diameter. Over mainland areas of north-west Wales faults were also caused by snow-laden trees making contact with overhead conductors.

MANWEB contacted the Meteorological Office at RAF Valley to see if we could offer an explanation of why damage was so bad on Anglesey where apparently snow accumulations were much less than on the mainland (by 09 GMT on 17 March 4 cm of snow had been measured at Valley compared with depths of over 40 cm at many places on the mainland).

### **Synoptic situation**

On 16/17 March a depression remained slow-moving near south-east England with a strong north to north-east airstream over much of the British Isles. Figure 1 shows the synoptic chart for 00 GMT on 17 March when the storm was at its height. The tephigram for Aughton at the same time (see Figure 2) should be representative of the air mass affecting Anglesey. Below 720 mb the air was very cold but was overridden at higher levels by much warmer air associated with the warm frontal zone which was moving slowly north over eastern England. The temperature of the surface air on the Lancashire coast was near to or a little below freezing (see Figure 3) but calculations indicate that the temperature of the cold air crossing the Irish Sea (sea temperature near 6 °C) should have risen to about 3.5 °C on reaching the north coast of Anglesey. At Valley temperatures remained between 1.0 and 0.5 °C because



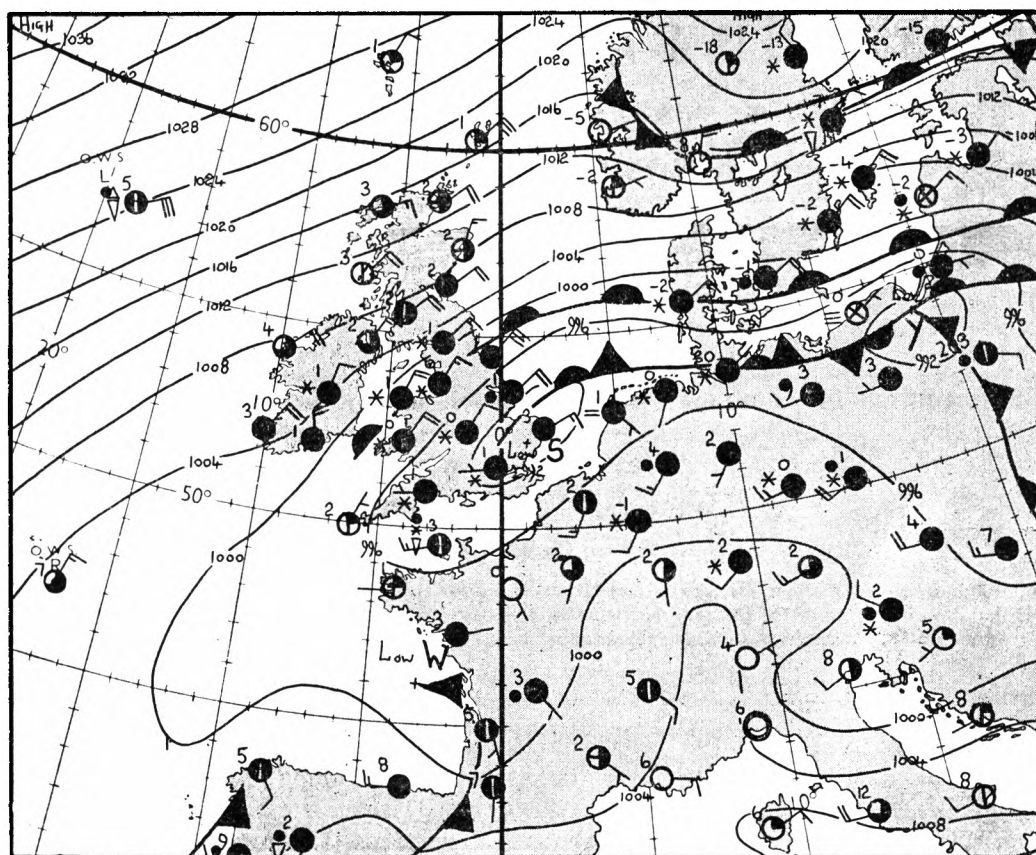


Figure 1. Surface synoptic chart for 00 GMT, 17 March 1979.

of the cooling effect of the falling snow and the distance away from the windward coast (about 25 km). The snowfall resulted mainly from warm air overriding cold, but was accentuated by convection over the Irish Sea.

### Observations at Valley

Snow showers commenced at Valley around 01 GMT on the 16th with the showers becoming more frequent until continuous precipitation was reported by 11 GMT. Snow of light or moderate intensity continued until 12 GMT on the 17th with further periods of light snow until 05 GMT on the 18th. Between 06 GMT on the 16th and 06 GMT on the 18th, an equivalent rainfall of 35 mm was recorded. Snow did not settle until dusk on the 16th and by 09 GMT on the 17th an accumulation of 4 cm was recorded, with equivalent rainfall from 18 GMT on the 16th to 09 GMT on the 17th of 21.7 mm. The water equivalent of this snow was 12.7 mm, a high value but perhaps typical of very wet wind-packed snow. On the night of 16/17th the dry-bulb temperature varied between 1.0 and 0.5 °C and the wet-bulb temperature between 0.7 and 0.4 °C. As would be expected in conditions of thawing snow, both grass and concrete thermometers registered minimum temperatures of 0.0 °C. The mean overnight wind was 030°, 10 m s<sup>-1</sup> with gusts 17.5 m s<sup>-1</sup>.



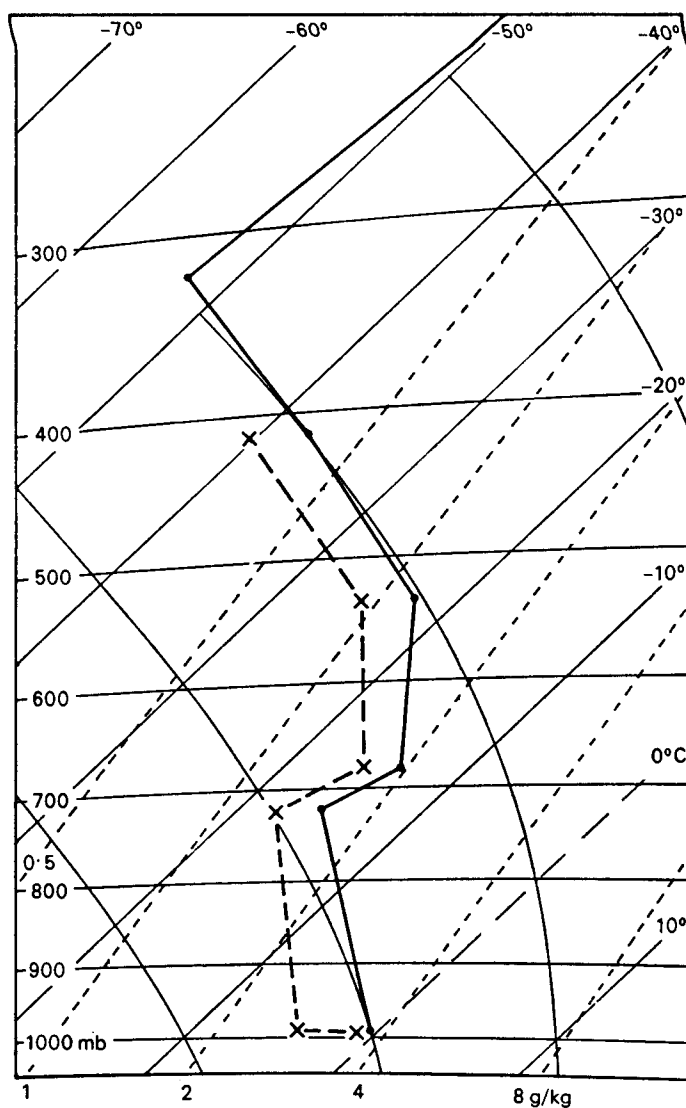


Figure 2. Tephigram for Aughton (near Liverpool) at 00 GMT on 17 March 1979.  
 · — · dry-bulb temperature; × — — × dew-point temperature.

The author, driving to the office on the morning of the 17th, observed that road surfaces were covered with wet snow with no sign of ice formation; however, on fences, poles and cables exposed to the strong wind there were considerable accumulations of ice-like, frozen wet snow. Numerous telegraph poles were down with others leaning perilously.

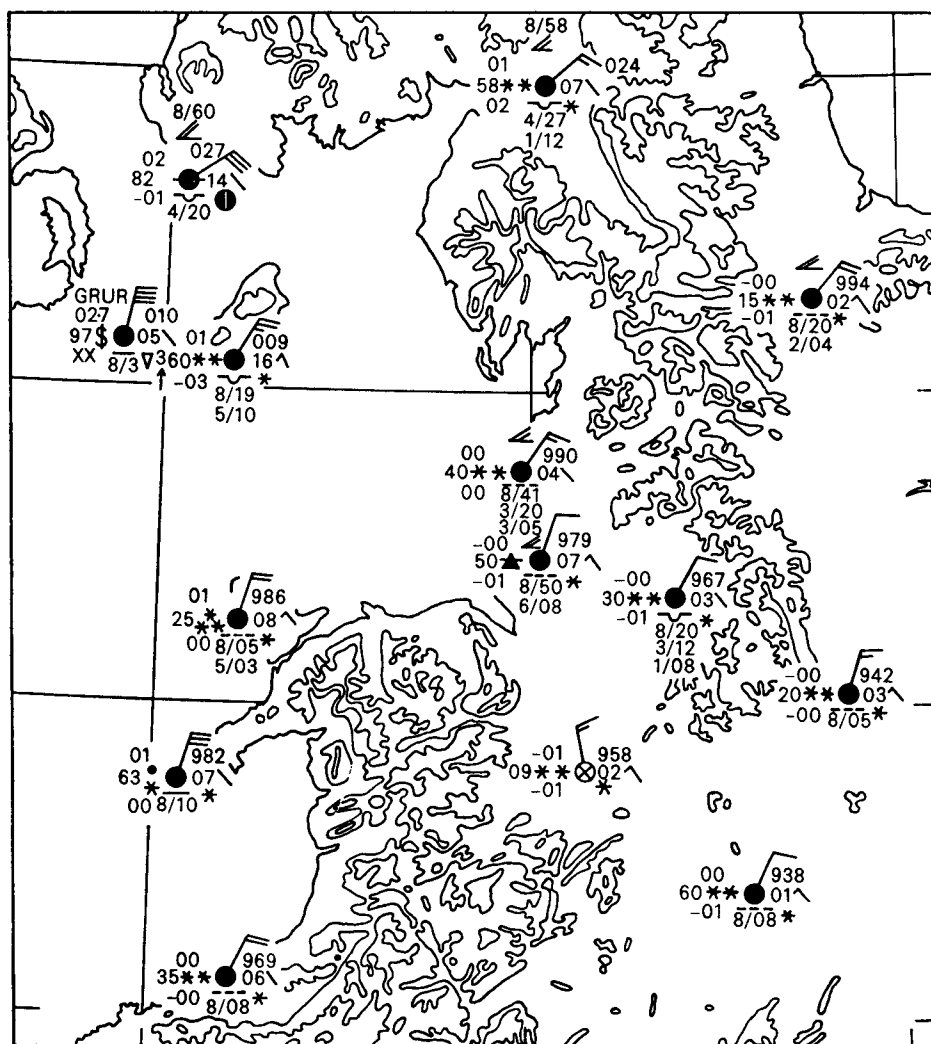


Figure 3. Plot of observations for 00 GMT, 17 March 1979. Contours are indicated at 200 m and 400 m.

## Discussion

The damage to overhead conductors obviously resulted from the accumulations of frozen wet snow and was made worse by oscillations in the conductors caused by sudden shedding of the ice load as a result of strong winds. Unlike rime icing, which commonly occurs in upland areas when a cloud sheet with its temperature remaining below zero results in water droplets freezing on impact to form heavy deposits of ice (Sarson 1956, Phillips 1956), damage caused by freezing wet snow is quite rare. However, Foot (1972) describes cases very similar to the events on Anglesey during 16/17 March 1979. He gives the necessary conditions likely to lead to severe icing on conductors as:

- (1) a dry-bulb temperature above 0 °C,
- (2) continuous moderate or heavy snow—suggested rate  $\geq 1 \text{ cm h}^{-1}$ , and
- (3) a strong surface wind—the higher the temperature the stronger the wind required to cause freezing.

Foot considered that the wet-bulb temperature was not important since, if the snowflakes were melting as they fell, they would continue to do so on the conductor and therefore evaporative cooling was not an important parameter. However, no satisfactory explanation is given of how the freezing on the conductor takes place.

The following mechanism is put forward to explain the occurrence. A snowflake falling into a layer with a temperature above 0 °C may begin to melt even when the wet-bulb temperature is a little below 0 °C. This is because, since the snowflake is blown by and hence to a considerable extent moves with the horizontal wind flow, and since the psychrometric constant is related to the rate of ventilation, cooling by evaporation will be relatively slow. When the snowflake impinges on a surface exposed to a strong wind, the wind flow relative to the snowflake will then suddenly increase by a large factor and strong cooling by evaporation will increase the likelihood of freezing. For refreezing to occur the value of the wet-bulb temperature for a particular wind speed will be very critical. If the wet-bulb temperature is too low the snowflake will not melt before impact.

At Valley during the period when freezing was taking place, the steep lapse rate of moisture content indicated on the Aughton tephigram would give wet-bulb temperatures below 0 °C at elevations only a little above screen level, although at screen level it remained above 0 °C. Over much of the mainland of north-west Wales wet- and dry-bulb temperatures were likely to have been at or below 0 °C with no icing problems. This was because of the generally elevated nature of the land compared with Anglesey, and also because the steep hills close to the north coast would prevent much incursion inland of air warmed over the Irish Sea.

Wet snow freezing on conductors can also cause problems with railway operations. Parrey (1970) describes a case when southbound train services between the Midlands and London were disrupted because the weight of ice and snow brought down the locomotive pantographs and prevented contact with the overhead lines. Little or no pantograph trouble was experienced with northbound trains. His explanation for the freezing of wet snow was that the trains had travelled from an area where temperatures were below 0 °C into an area with temperatures above freezing and that the wet snow then froze on the pantographs which were sub-zero. However, Ludlam (1951) shows that the thermal capacity of a conductor is not a significant heat source or sink (a typical conductor of radius 0.5 cm and at a temperature of  $-5 \text{ °C}$  can only freeze a layer of ice  $2 \times 10^{-2} \text{ cm}$  thick, assuming that the conductor only gains heat from freezing water at 0 °C).

It is suggested that cooling by evaporation when wet-bulb temperatures were just below 0 °C was again the cause. In the case of a moving train there is no need for a strong surface wind since the train will be in an apparent wind. This suggests that on a day with no wind, ice might form on the moving pantographs but not on overhead lines. With a strong surface wind the situation would be more complicated. For example a train moving against a strong wind would be more liable to icing than a train moving with the wind.

## Conclusion

Wet snowfalls occur in most winters and yet the formation of ice on conductors is uncommon. For freezing of wet snow to occur certain conditions must prevail and as indicated the wet-bulb temperature

is the most critical of the parameters. It is suggested that when the other conditions as described by Foot (1972) are met, ice formation on conductors should be forecast if the wet-bulb temperature is close to 0 °C.

### Acknowledgement

Thanks are due to the Chief Engineer's Department of MANWEB for details concerning the damage to their overhead conductors.

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## **A note concerning progress and plans for the establishment of operational networks of quantitative weather radars**

By C. G. Collier

(Meteorological Office Radar Research Laboratory, RSRE, Malvern)

### **Summary**

An account is given of work now being undertaken throughout the world on merging data from several quantitative weather radars in order to provide an operational service to a wide variety of users. Some mention of future plans is made when these are known to the author.

The uses to which quantitative radar data may be put are wide-ranging, including both operational and research applications (Bussell *et al.* 1978, Browning 1978). The accuracy with which a radar may measure rainfall amounts has received particular attention over the last decade. Recently, however, the emphasis has moved towards the implementation of a number of quantitative weather radars working together as a network. This note contains a summary of the work currently being undertaken throughout the world on the merging of data from more than one quantitative weather radar for the purpose of providing an operational service to a variety of users ranging from hydrologists to those concerned with weather forecasting for aviation. Some mention of future plans, where they are known to the author, is also included. Most of the information in this note was gleaned when the author attended the Symposium/Workshop on Digital Radar Reflectivity Processing with Applications to Hydrometeorology held in Edmonton, Alberta, Canada, from 15 to 18 October 1979\*. The countries mainly involved in developing radar networks are noted below. Some work, particularly in eastern Europe, was not discussed at the Edmonton Symposium, and will not be described in detail here.

*Canada.* An operational network of C-band (5.6 cm wavelength) radars has been installed, using software and hardware technology developed over the last 20 years by the Physics Department, McGill University, Montreal. This network covers the more highly populated parts of southern Canada. By the beginning of 1980 users will be able to dial in to the individual radar systems to obtain data in real time. During the next decade it is intended to expand the network and replace the older existing radar systems (a technique of deriving rainfall information from geostationary satellite data (Lovejoy and Austin 1979) will also be introduced operationally). It is likely that the interchange of radar data across the US-Canadian border will be given some consideration during the next few years. It is planned to devote considerable effort during the 1980s to the development of applications software and to the education of users of the new products.

*Japan.* The first of a new generation of quantitative weather radars came into operational use in 1977 at Mount Akagi under the control of the Japanese Ministry of Construction, who have supervised the installation of two further systems which began operation during 1979 (at Mount Shaka and Mount Mitsutoge). During the summer of 1979 data from two of the radars were merged in real time to form a composite picture which could be displayed on a colour display system. The intention is to install over the next decade a national radar network consisting ultimately of about 16 radars.

*United Kingdom.* Work with quantitative weather radars is centred upon the Meteorological Office Short-Period Weather Forecasting Pilot Project (Browning 1977) and the North West Weather Radar

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\* The proceedings of the Symposium/Workshop may be published during 1980.

Project (Collier *et al.* 1980). This work is based upon the achievements of the Dee Weather Radar Project (Central Water Planning Unit 1977), and work carried out at the Royal Signals and Radar Establishment (Ball *et al.* 1976, 1979). A research network of four weather radars has been established, and data from these radars have been merged in real time and displayed on a colour interactive display system for use by a team of forecasters (Collier 1979). The philosophy underlying this system, known as the FRONTIERS (Forecasting Rain Optimized using New Techniques of Interactively Enhanced Radar and Satellite) strategy, has been described by Browning (1979).

*United States.* There are some 70 operational weather radars in the United States at the present time. Of these only four are fully digitized, following the work carried out in the D/RADEX (Digitized RADAR EXperiments) Project (McGrew 1972, Saffie 1976). The plan to digitize the remaining radars, known as RADAP (RADAR Analysis Project), involves the placing of a commercial contract during 1981–82 (NOAA 1978). A project, similar to the UK Short-Period Weather Forecasting Pilot Project, and known as HRAP (Hydrologic Rainfall Analysis Project), is currently being formulated. This Project is aiming to bring together rain-gauge, radar and geostationary satellite data in near real time, either via the existing National Weather Service AFOS (Automation Field Operations and Services) (Klein 1976) communications network, or, more likely, via river forecast centres at a central compositing computer.

*Federal Republic of Germany.* A C-band radar has been in operation at the German Weather Service Meteorological Observatory, Hohenpeissenberg (Upper Bavaria) since 1974, as part of a research program (Anderl *et al.* 1976) similar to the Dee Weather Radar Project. During 1980 the intention is to bring into operational use a network of seven radars. Data from these radars will probably be merged to produce three composite pictures every ten minutes, one covering the north of the country, one the central part of the country, and one the south of the country.

*Other countries.* Sweden has a network of radars which it is planned to update in the near future. Several countries in eastern Europe, notably the Soviet Union (Chernikov 1976) and Poland (Kawecki 1977) have established weather radar networks over the last decade, although most of the radars are not being used quantitatively. Plans to interchange data between east European countries have also been formulated (Ziemann 1973).

From the above it is clear that there is a lot of activity in the implementation of quantitative weather radar networks in many countries. In certain countries, notably Japan and Canada, plans for the operational use of quantitative data from a radar network are well advanced, and data are already being made available to a variety of users on a fully operational basis.

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## **The use of satellite pictorial data in weather forecasting**

By J. G. Moore

(Meteorological Office, Bracknell)

### **Summary**

The Central Forecasting Office of the Meteorological Office uses pictorial data from both polar orbiting and geostationary satellites to help the forecaster in the production of more accurate representations of pressure and humidity fields and frontal positions by means of analyses of clouds and cloud systems. The data can also be used to provide running checks on how well numerical forecasts are performing in the first 6 to 12 hours.

### **1. Introduction**

Meteorologists have been using satellite pictorial data for a number of years as aids for analysing synoptic and smaller-scale weather features. Their use and importance have increased with the continued improvement of satellite instrumentation and because of the extra dependence placed on them following the reduction in the number of ocean weather stations making surface and upper-air observations.

In regions where more conventional types of surface and upper-air observations are few or lacking (i.e. oceanic areas away from the main shipping routes) satellite pictures may at times provide the only current or recent evidence on a particular weather system.

The analysis and forecast areas for the coarse-mesh version of the Meteorological Office 10-level model cover much of the northern hemisphere. The Meteorological Office provides forecast information for many parts of the northern hemisphere and it is thus important that the basic analysed fields are the best possible for the whole area. Numerical forecasts are computed routinely for periods up to six days ahead and sometimes the longer-term predictions for the British Isles area may depend significantly on initial analyses for quite distant areas. Satellite pictures for all parts of the northern hemisphere are thus of potential importance to the work of the Central Forecasting Office (CFO).

### **2. Pictorial satellite data available**

Pictorial satellite data used in CFO fall broadly into five categories:

1. Visible and infra-red data from the US polar orbiting satellites TIROS N and NOAA 6 received in near real time (received at Lasham and relayed to Bracknell).
2. Visible and infra-red data from the ESA geostationary satellite Meteosat\* received in near real time (relayed from Lasham to Bracknell).
3. Visible and infra-red data from the GOES E geostationary satellite at 75°W and from the GOES INDIAN OCEAN geostationary satellite at 58°E received in near real time (relayed via Meteosat through Lasham to Bracknell).
4. Visible and infra-red data from orbiting and geostationary satellites (mostly in mosaic form) received over normal international meteorological facsimile networks from various parts of the world. These include data on tropical storms (not in real time).

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\* This article was written before the failure of Meteosat in late November 1979.



5. Composite mosaics derived from visible and infra-red data from the US polar-orbiting satellites received by Lasham. These are prepared in the Meteorological Office Systems Development Branch (Met O 22) in near real time.



Figure 1. Approximate area from within which satellite images from TIROS N and NOAA 6 can be received at Lasham.

### *TIROS N and NOAA 6*

Continuously broadcast signals from an orbiting satellite can be received at a station on the earth's surface while the satellite remains above the horizon from the receiving point. In the case of TIROS N and NOAA 6, orbiting on tracks passing close to both north and south poles at respective heights of approximately 870 and 830 km, the area of the earth's surface which may be surveyed using signals received at Lasham in real time is approximately that shown in Figure 1. This area comprises much of Europe (including the British Isles), Scandinavia, North Africa, Iceland, east Greenland and a large part of the Atlantic of middle and northern latitudes. This is the area probably of most interest to forecasters when forecasting for the United Kingdom for periods of 24–36 hours ahead. Each satellite

scans the area twice daily with three (sometimes four) successive satellite passes, each pass occurring about 25° of longitude further west and 102 minutes later than the previous one. Since the satellite does not complete an integral number of orbits in a day the tracks do not repeat on a daily basis although the local solar time is essentially unchanged for latitude crossings. Spatial resolution is about 4 km in both visible and infra-red channels. On-board satellite processing of the imagery produces a linearized picture, i.e. reduces the crowding of lines of longitude towards the picture edges evident with earlier satellites.

Sectors of these pictures showing particular areas on a larger scale (in the Meteorological Office case, the British Isles area) are also available. Picture resolution for these is about 1 km in both visible and infra-red channels. Central Forecasting Office analysts add grids of latitude and longitude to the satellite pictures using an overlay appropriate to the notified orbit characteristics.

### *Meteosat*

Geostationary satellites, of which Meteosat is one, orbit the equator at a height of 35 000 km remaining effectively above the same point on the equator (0°E for Meteosat) from which they continuously view the same area of the earth's surface.

For Meteosat thermal infra-red data the disc viewed is divided into nine roughly equal areas, D1 to D9 (see Figure 2(a)), D2 being the most important format for analysing the western Europe/east Atlantic sector.

For Meteosat visible data the disc is subdivided into 24 areas, C1 to C24 (see Figure 2(b), formats C2 and C3 being the most important for the western Europe/east Atlantic sector. Coast lines and grids of latitude and longitude are superimposed on most formats by the ESA station at Darmstadt. Meteosat has the facility to provide at very frequent intervals both infra-red and visible pictures for particular D and C formats which can be particularly useful for tracking such features as rapidly moving waves in a jet stream for instance. Drawbacks are the oblique view that is taken of the earth in higher latitudes (e.g. northern Europe and North Atlantic) and image resolutions inferior to those of the polar-orbiting satellites.

Meteosat also transmits image data in the water vapour absorption band on formats E1 to E9 (E1 covering the same area as D1 etc.; see Figure 2(a)) which are representative of the humidity of the middle troposphere (600–300 mb). Image data from the US GOES geostationary satellites at 75°W and 58°E are also relayed to Bracknell via Meteosat.

### *Miscellaneous*

Other miscellaneous satellite pictures and composite mosaics are relayed from various national sources via the international facsimile networks. These are usually some hours old when received and because of transmission and reception conditions lack the gradations of shade from black to white obtained with pictures received in real time via Lasham.

### *TIROS N and NOAA 6 composite displays*

Signals from TIROS N and NOAA 6 are also relayed to the Meteorological Office Systems Development Branch where a composite picture is prepared from the three or four passes received at Lasham. The pictures are merged where they overlap and are reproduced on a  $1:20 \times 10^6$  polar stereographic chart to which lines of latitude and longitude and coastlines are added. The fact that this is on the same scale and projection as the Senior Forecaster's chart allows direct comparisons to be made using a light-table.

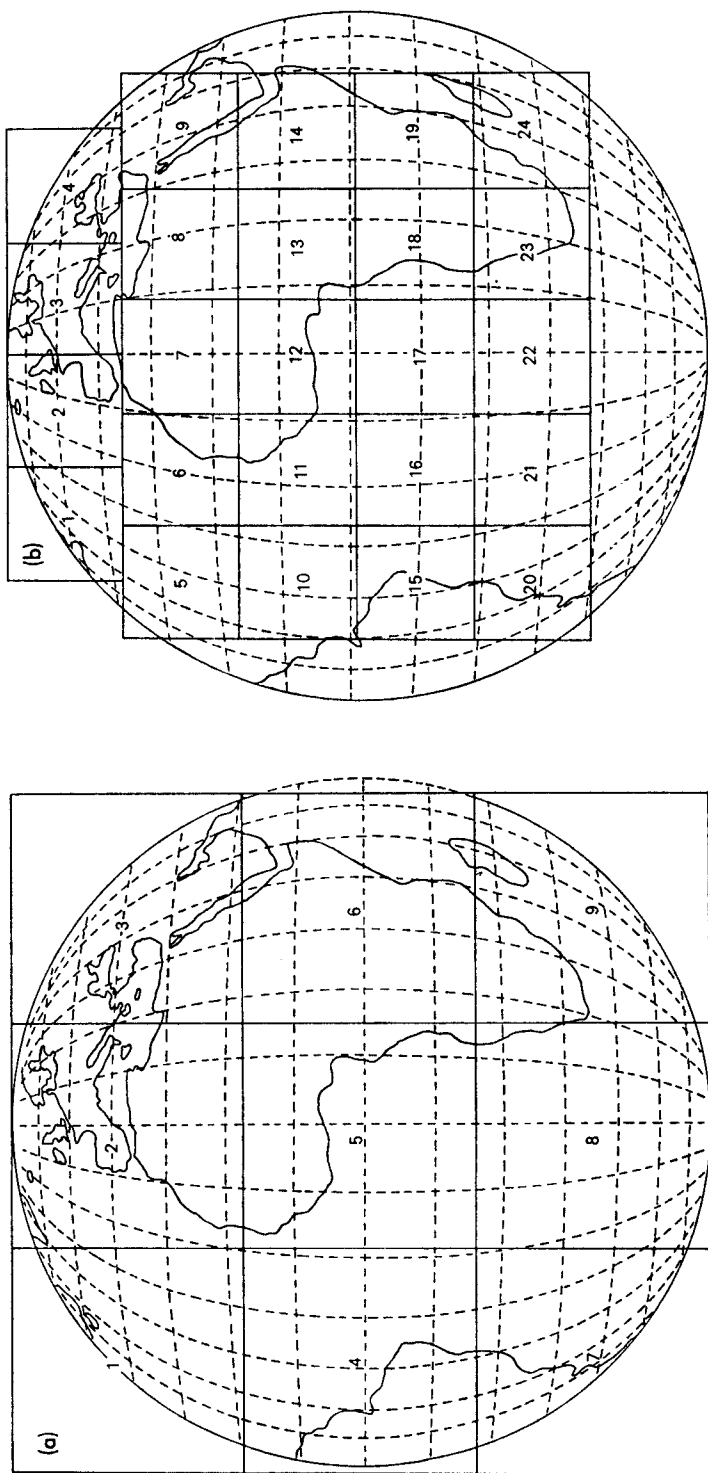


Figure 2. Area viewed by Meteosat: (a) infra-red—9 subdivisions (b) visible—24 subdivisions.

### 3. The use of satellite pictorial data

Satellite pictorial data (visible and infra-red) are used for synoptic analysis in a somewhat indirect way through the analysis of clouds and cloud systems.

As well as showing the size, location and shape of areas of cloud, visible and infra-red satellite pictures can, from an examination of the relative brightness and texture of the images, provide not only useful information on the cloud types present but also on their vertical structure on both synoptic and smaller scales. Brightness of a cloud image on a visible picture depends on the sun's illumination, the reflectivity (related to cloud thickness) and the relative positions of cloud, sun and radiometer. With an infra-red picture the brightness depends on the temperature of the emitting surface (i.e. the brighter the image the colder (higher) the cloud top). At night, of course, only infra-red data are received.

Cold fronts and occlusions are usually well defined on satellite pictures though warm fronts are not so easily discernible (except in the early stages of development before they become overlain with cirrus). Spiralling cloud patterns indicate the positions of developed vortices though clouds over a depression at the wave stage rarely show a circulation. Satellite pictures often show the development of a wave on a cold front which proves very useful for areas where conventional observations are few. Waves commonly develop on a cold front when a comma-shaped cloud (denoting an area of positive vorticity advection) in the cold air moves close to the front. The frequency of the Meteosat pictures (at hourly or shorter intervals) helps to confirm tentative conclusions drawn from a subjective interpretation of an individual picture, particularly for systems approaching the British Isles from the west and south-west. Plates I–VI show a developing wave moving rapidly north-east from west of Iberia to the British Isles. Figures 3 and 4 show synoptic charts for comparison with Plates I and V respectively. Satellite pictures are also valuable in locating tropical storms. Patterns formed by individual cells (e.g. groups of cumulus) can also be helpful in indicating the position and intensity of synoptic features. Upper troughs, cold pools and ridges may be located from the evidence of satellite pictures and sometimes jet streams also (from the shadow of jet-stream cirrus on cloud below or from cirrus streaming ahead of a developing wave). Satellite pictures are sometimes useful for establishing links between different systems, especially new links. However, care is required as links have sometimes become inactive and bands of low cloud which are not dynamically significant are best omitted from disseminated analyses.

On the smaller scale, satellite pictures are used to help make short-term forecasts of the persistence or dissipation of fog and stratus. Early-morning visible pictures can reveal the extent and boundaries of fog and stratus common to high-pressure systems. Variation in the brightness of the top of visible pictures gives an indication of the thickness of fog, the more persistent areas appearing the brightest. Satellite pictures are also useful in deciding the extent of fog and stratus over sea areas.

Limited use has been made of the Meteosat water vapour formats (E1 to E9). On these, white regions indicate a relatively moist upper troposphere and dark regions one that is relatively dry. Some inferences may be made of the rising and sinking motions in upper flows and the pictures have sometimes been useful for delineating weak linkages between fronts and jet-stream flow.

### 4. Relative humidity analyses

The functions of the 10-level model include the production of forecast fields showing the expected rate of rainfall (mm/hour) and expected rainfall amounts in 6-hour periods (mm) over the areas covered by the coarse-mesh and fine-mesh versions. Particular importance is attached to rainfall forecasts produced by the fine-mesh version for the area around and including the United Kingdom.

Objectively analysed fields of relative humidity for the 1000, 850, 700 and 500 mb levels are produced by the computer to provide the model with the necessary data on the initial moisture content of the lower troposphere.

Over meteorologically well-documented areas, such as Europe, surface and radiosonde observations provide sufficient data for an adequate relative humidity analysis. Over large sea areas, it is often difficult to form an adequate relative humidity analysis from the few widely spaced radiosonde ascents available. It is often particularly important for rainfall forecasting for the United Kingdom that the relative humidity analysis for the Atlantic area west of the British Isles be as accurate as possible. In the Central Forecasting Office an attempt is made to supplement the small amount of conventional observational data available from the Atlantic area with assessments of relative humidity made from satellite pictorial data.

The pair of orbiting satellite visual and infra-red pictures covering the UK and the Atlantic area west of the British Isles received during the morning is used as the guide for the 12 GMT relative humidity fields. Such a pair can provide a useful indication of the areas and levels of highest and lowest humidity. At night, for the 00 GMT analyses, satellite guidance can only come from the infra-red pictures, so daytime necessarily is the main time for humidity analysis modification. Meteosat pictures close to the main observation time may provide supplementary information.

Normally, the shapes of cloud areas are transferred manually from the satellite pictures to a copy of the Senior Forecaster's latest working chart (for midday humidity modification the 06 GMT chart is usually used). On this is indicated the estimated horizontal and vertical extents and thicknesses of cloud (height crudely estimated as high, medium or low). This is done in conjunction with available surface observations (since rain-bearing clouds may be overlain by extensive cirrus) making allowance for differences between chart and satellite observation times. When considering relative humidity analyses for the 10-level model, 500 mb is normally taken as the level for cirrus, 700 mb for medium-level cloud and 850 and 1000 mb for low cloud and fog.

The analyst will check that the positions of the areas of high and low relative humidity on the background field (i.e. the 12-hour forecast of relative humidity which is used as a first guess to the analysis) are consistent with what is shown by the satellite pictures and that the values of relative humidity themselves seem to be of the expected order. If there are inconsistencies, he has two courses of action. He can alter the background field, displayed as isopleths on a visual display unit (VDU). These isopleths can be altered to fit the preferred analysis (superimposed on a plastic overlay) by using a light-pen, the final amended analysis then being fed back to the computer. Alternatively, invented values of relative humidity for particular positions (bogus observations) may be fed to the computer to produce the required analysis.

Choice of values of relative humidity to use is a very subjective process. Estimates are usually made to the nearest 5 or 10 per cent, the aim being to give a reasonable three-dimensional description of the humidity structure in a particular system rather than aiming for precision at a particular level. Particular attention is devoted to systems that are thought to pose a possible chance of bringing rain to the United Kingdom area within the following 24–48 hours. It is generally considered unwise to adopt values as high as 100 per cent. Use of these values at successive standard levels lying in cloud could imply a thick column of totally saturated air which may not be representative of the column as a whole.

It has been found best to use values usually no higher than 85–90 per cent and rely on vertical motions computed by the model and typical of rain-producing systems to achieve the levels of saturation required for rainfall. (Radiosonde ascents rarely indicate 100 per cent relative humidity over columns of great extent, even in rain-bearing conditions.)

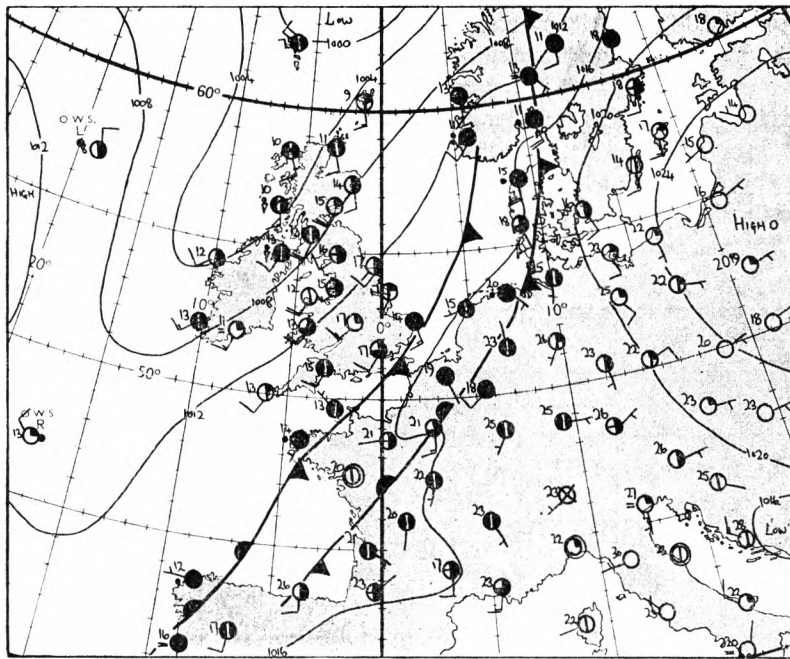


Figure 3. Surface synoptic chart for 18 GMT, 29 May 1979.

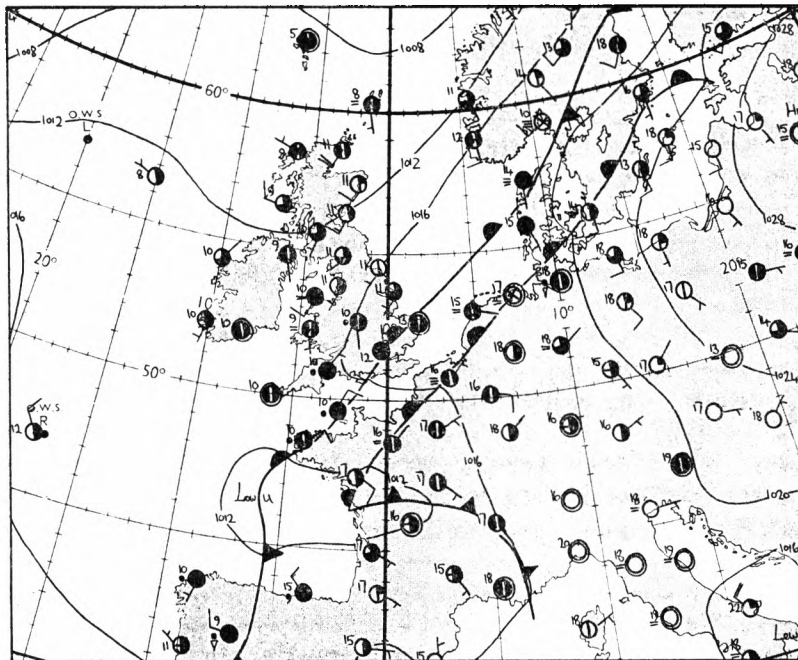


Figure 4. Surface synoptic chart for 06 GMT, 30 May 1979.

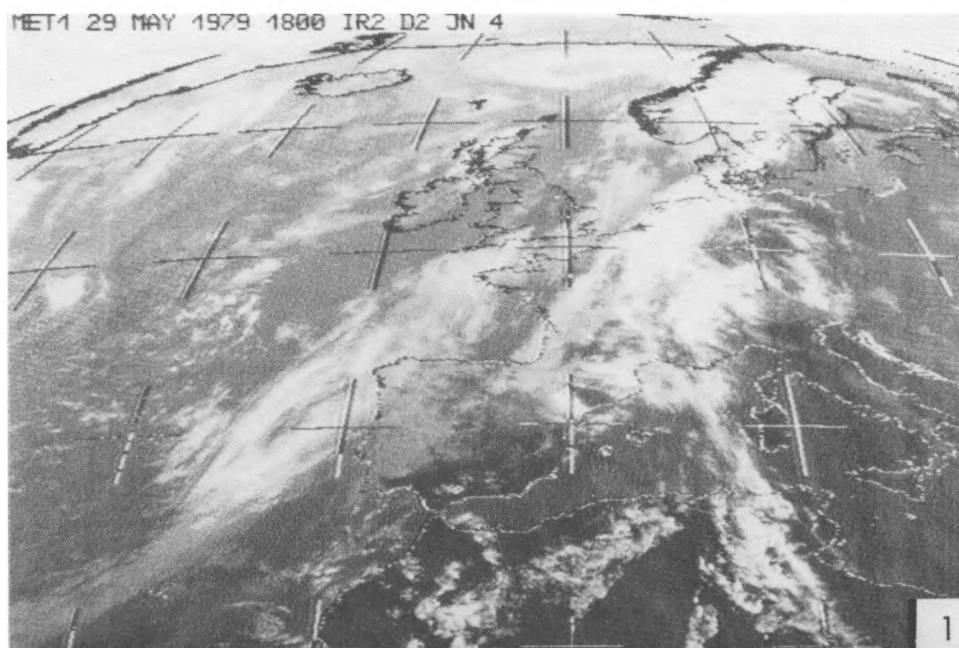


Plate I. Meteosat infra-red image for 18 GMT, 29 May 1979 (see page 82).

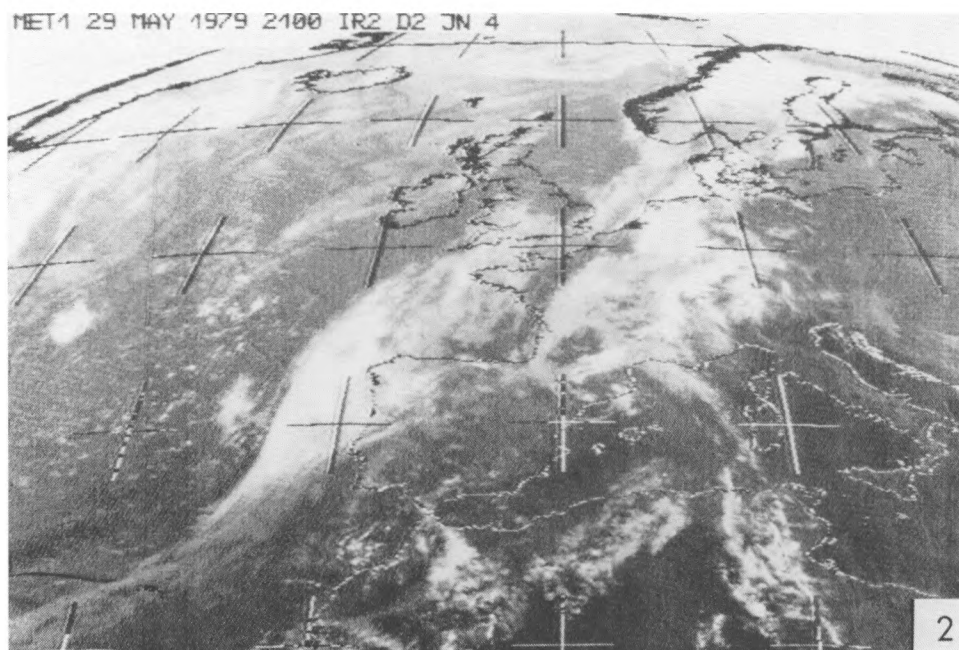


Plate II. Meteosat infra-red image for 21 GMT, 29 May 1979.

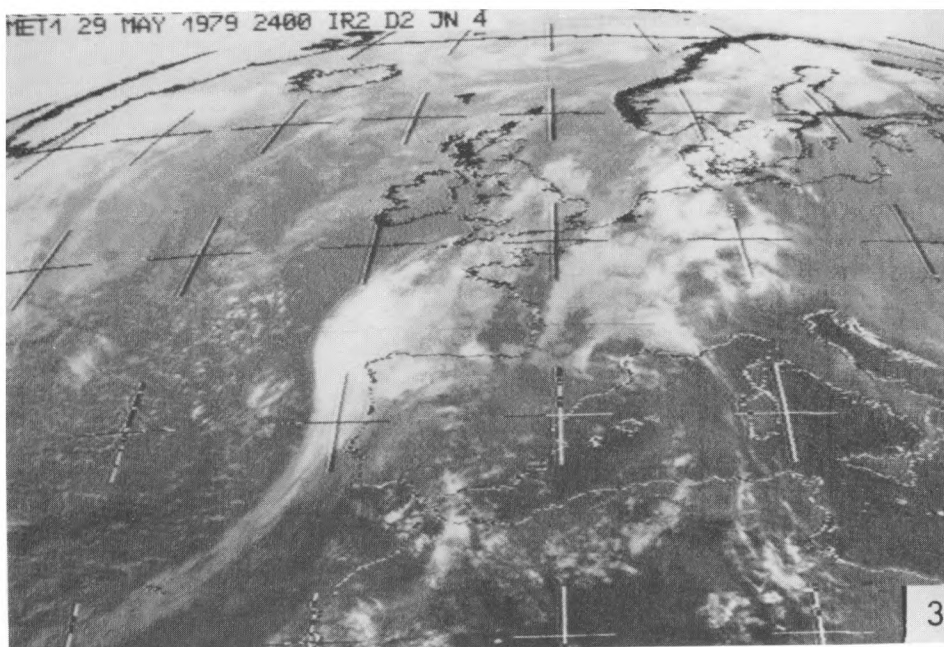


Plate III. Meteosat infra-red image for 00 GMT, 30 May 1979.

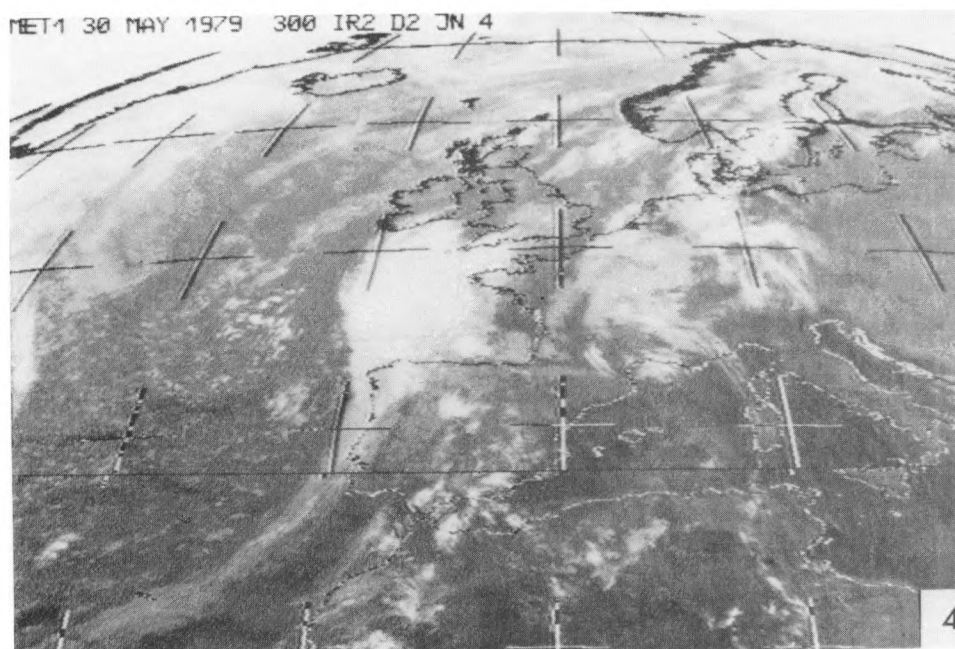


Plate IV. Meteosat infra-red image for 03 GMT, 30 May 1979.



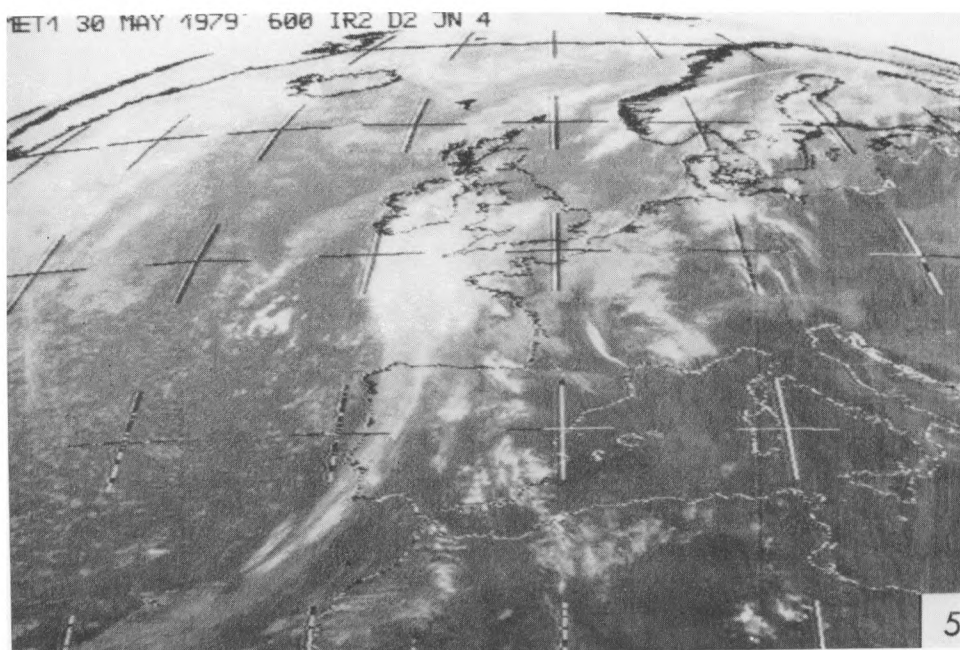


Plate V. Meteosat infra-red image for 06 GMT, 30 May 1979.

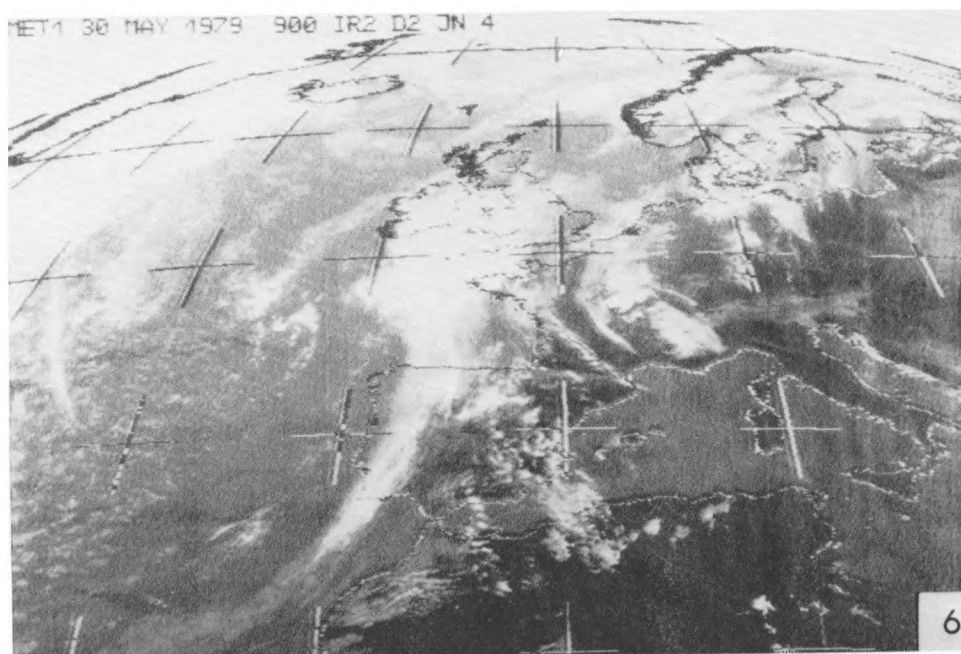
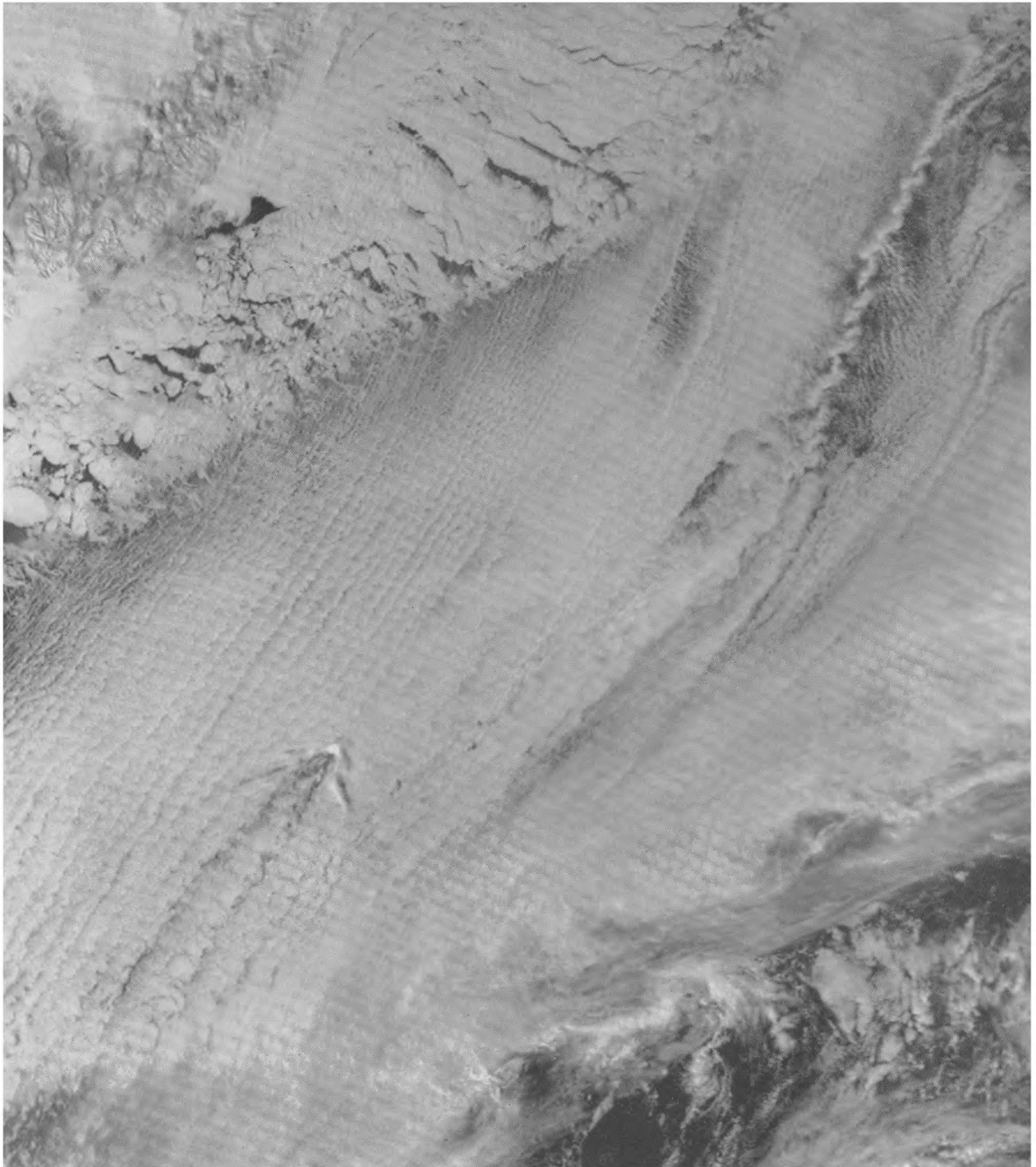
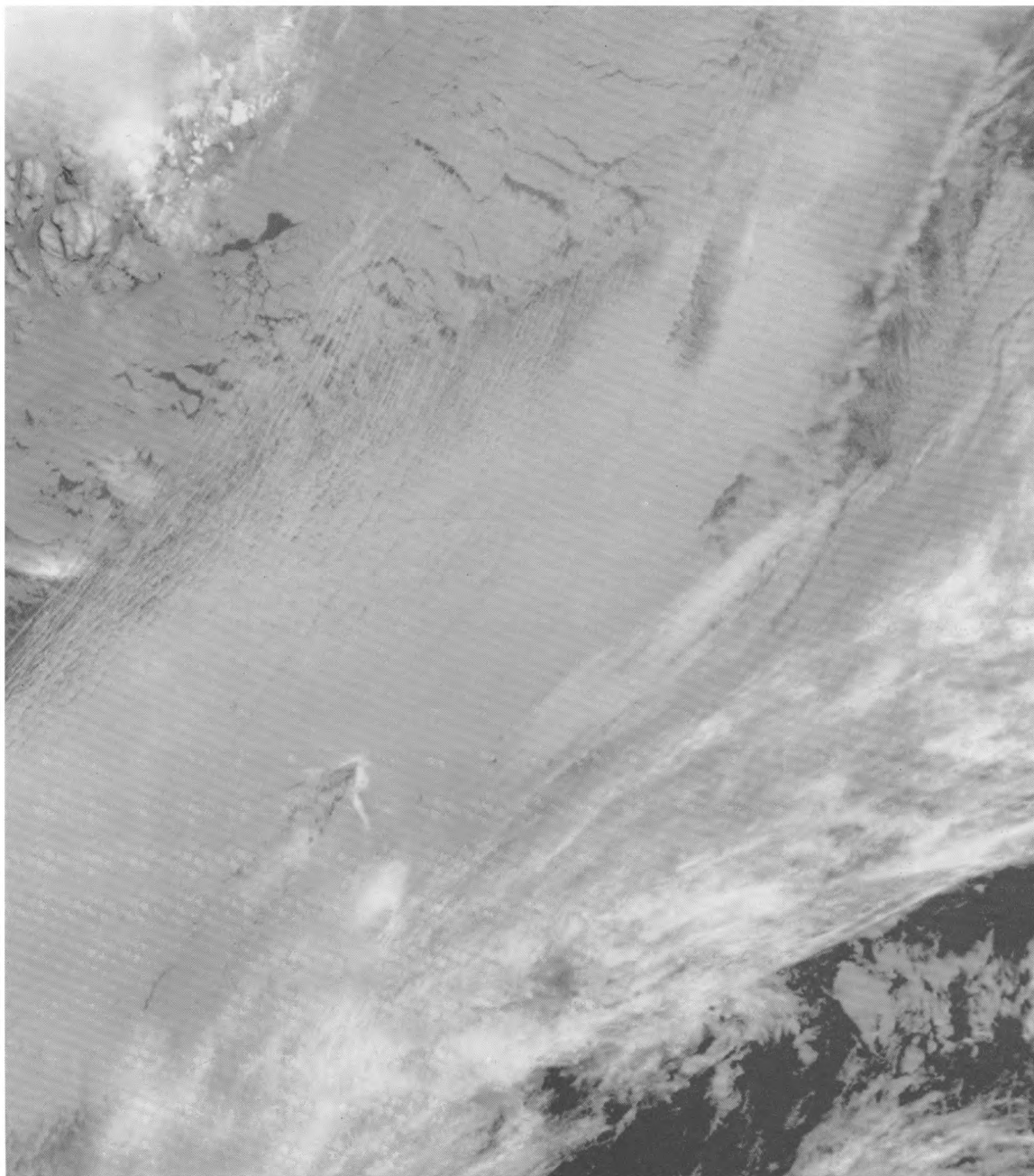


Plate VI. Meteosat infra-red image for 09 GMT, 30 May 1979.



*Photograph by courtesy of Dundee University.*

Plate VII. Visual image received from TIROS N satellite at 1407 GMT, 29 April 1979. The cloud resembling a ship's bow wave locates Jan Mayen Island; large ice floes are seen off the east coast of Greenland with pack ice to the north, and at the top right there is a shear rope cloud. (See page 85.)



*Photograph by courtesy of Dundee University.*

Plate VIII. Infra-red image from TIROS N satellite for the same time as the visual image shown as Plate VII.



Plate IX. L. G. Groves Memorial Prize and Award winners with Mr Nicholas Abbott, Air Marshal Sir John Nicholls and Air Commodore K. W. Hayr. Seated left to right: Mr Nicholas Abbott, Air Marshal Sir John Nicholls, K.C.B., C.B.E., D.F.C., A.F.C., and Air Commodore K. W. Hayr, C. B. E., A.F.C. Standing left to right: Flight Lieutenant J. S. Garnons Williams, Mr D. E. Miller, and Flight Lieutenant J. Holland. (See page 88.)



Plate X. Mr Nicholas Abbott and Mr D. E. Miller, winner of the Meteorology Prize.





Plate XI. Mr Nicholas Abbott congratulates Flight Lieutenant J. Holland, winner of the Meteorological Observer's Award.



Plate XII. Mr Nicholas Abbott presenting the Second Memorial Award to Flight Lieutenant J. S. Garnons Williams.

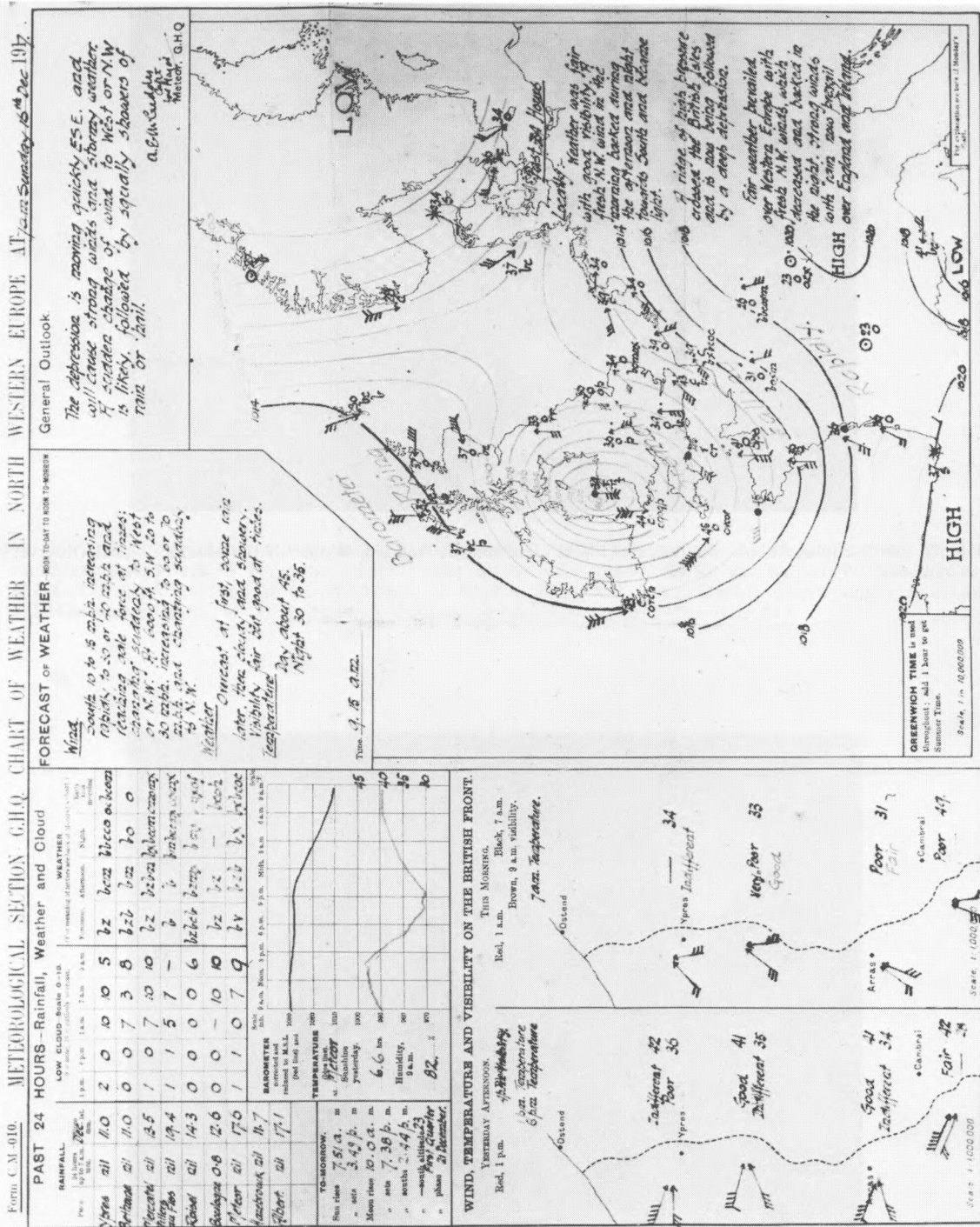


Plate XIII. See page 95.

## **5. Assessment of fine-mesh forecasts**

Satellite pictures, especially those from Meteosat, are sometimes useful in judging whether the evolution of the fine-mesh rainfall forecasts is likely to be successful. Usually, the Senior Forecaster, during the early afternoon when preparing his forecasts for the following day, relies for guidance on numerical data based on the previous midnight's observations (output based on 12 GMT data not being available until later in the afternoon).

The fine-mesh version produces forecast fields (including rate of rainfall) for the Atlantic/western European sector for 06, 12 and 18 GMT on that day together with forecasts for 00, 06 and 12 GMT the following day.

The forecaster can compare the forecast rate of rainfall field for 12 GMT on the first day with the cloud patterns shown on satellite pictures for near that time (usually Meteosat 11 or 12 GMT, whichever is available).

If the forecast rate-of-rainfall field seems consistent with the estimated area of rainfall from the satellite picture, extra confidence will probably be felt in the subsequent stages of the fine-mesh forecast. Significant differences may be attributed to:

- (i) timing errors in movement or development of the synoptic pattern;
- (ii) errors in forecasting the extent and intensity of rain.

It may then be possible at times with knowledge of the above to make a subjective modification and improvement to the later stages of the forecast.

Similarly, it is sometimes possible to compare the positions of identifiable synoptic features from the 12 GMT Meteosat pictures (lows, cold pools, troughs etc.) with their forecast position for the same time on the corresponding fine-mesh forecast, when only few or no synoptic observations for that time have been received. Again, this sometimes allows a successful modification (e.g. timing adjustment) to be made to the later stages of the forecast.

551.507.362.2:551.576.1

## **Interesting cloud formation seen on satellite imagery**

By N. J. Atkins

(Meteorological Office, Bracknell)

### **Summary**

Two interesting cloud phenomena seen on one satellite picture are briefly discussed and related to the weather situation at the time.

### **Introduction**

Plate VII shows a satellite (TIROS N) visual image received at 1407 GMT on 29 April 1979 and the corresponding infra-red image is seen on Plate VIII. Near the centre of the pictures is a cloud formation which has the appearance of a ship's bow wave, and this locates Jan Mayen Island. At the top right of each photograph there is a long twisted cloud best described as looking like a piece of rope.

**Synoptic situation.** On 29 April 1979 a depression to the south-east of Jan Mayen moved slowly eastward and with pressure building over eastern Greenland a strong northerly airstream covered a large area by midday (see Figure 1). At 12 GMT the Jan Mayen weather station (29 ft above mean sea level) reported a total cloud cover of stratus with base between 200 and 300 m (600–1000 ft), a northerly surface wind of 45 knots, air temperature  $-5^{\circ}\text{C}$ , dew-point  $-10^{\circ}\text{C}$  and visibility 100 m in heavy blowing snow (i.e. above eye level). There was no radiosonde release at midday but the ascent at 00 GMT (see Figure 2) represents the northerly airstream in early stages of development. The air is stable, suggesting the formation of turbulent stratus and stratocumulus in a strong airflow.

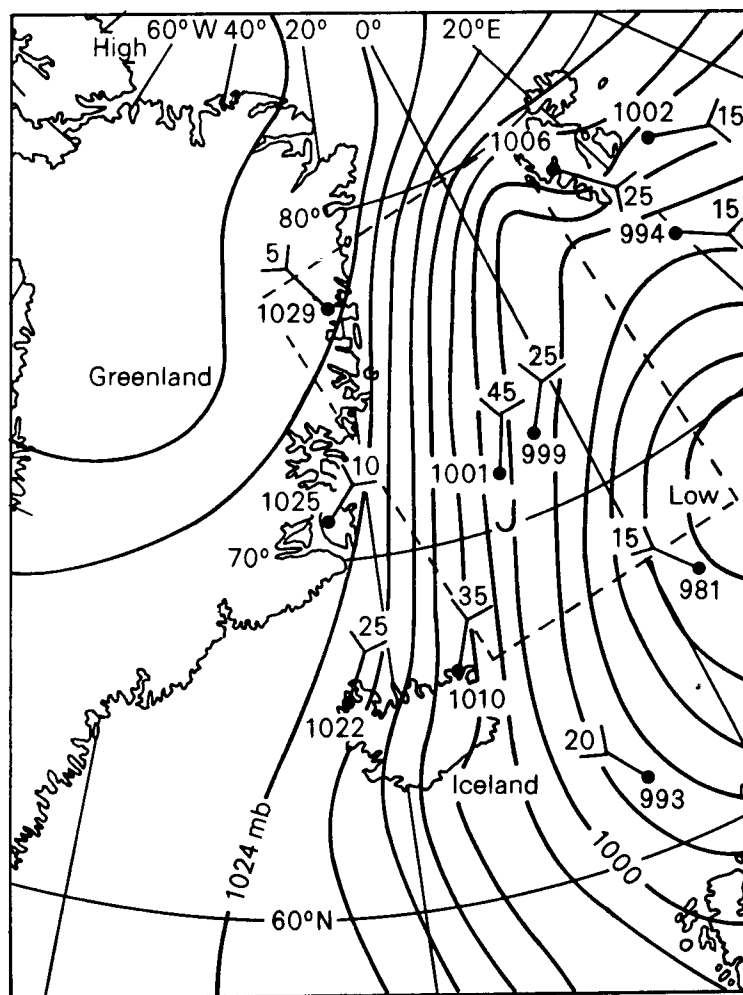


Figure 1. Surface analysed chart for 12 GMT, 29 April 1979 showing a strong northerly airstream over Jan Mayen Island (J). The area contained by the dashed lines is approximately the same as seen on the satellite pictures in Plates VII and VIII. Plotted observations show mean-sea-level pressure in millibars and wind speeds in knots.



**The bow wave cloud.** Wave clouds caused by obstacles are a reliable source of information on airflow direction in the lower and middle troposphere (WMO 1973, p. 219). If the obstacle is elongated the waves are usually aligned perpendicular to the direction of oncoming flow. Wave clouds formed by isolated islands, such as Jan Mayen, which is a volcanic island with the highest peak (Beerenberg) 2276 m (7469 ft), assume the appearance of a ship's bow wave spreading out on the leeward sides of the high ground. The direction of flow coincides with the bisector of the angle at which the wave clouds diverge. In the example illustrated the brightness of the wave cloud on the infra-red image (Plate VIII) indicates a higher cloud top due to forced lifting of the air mass up the mountain sides and the deeper cloud then flowing out along the wave. The shadow of the wave cloud can be seen on the visual picture (Plate VII).

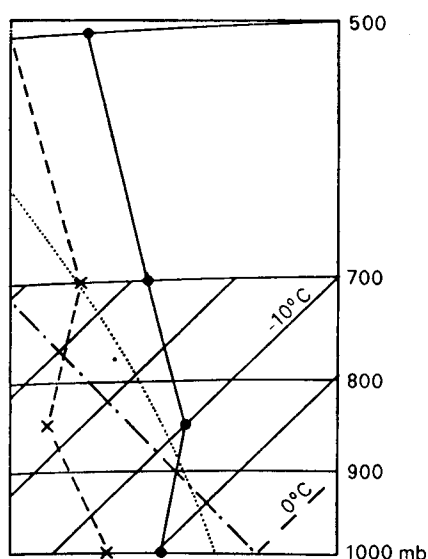


Figure 2. Radiosonde ascent from Jan Mayen at 00 GMT, 29 April 1979.  
 · — · dry-bulb temperature; × — — × dew-point temperature;  
 — · — · dry adiabatic; · · · · saturated adiabatic.

**The shear rope cloud.** A long spiral-like formation of cloud looking very much like a piece of heavy rope. The one seen in Plates VII and VIII is well shaped for a distance estimated to be about 150 miles. This type of cloud indicates turbulence and usually occurs in cold air; it does not require an upwind obstacle but a condition for its formation is a marked horizontal wind shear (Air Weather Service 1969, p. 5-D-3). A low-level wind shear is evident in this example (see Figure 1). The term shear rope cloud is used to differentiate it from rope clouds associated with tornadoes (Scorer 1972, p. 159).

**Acknowledgement.** I have to thank the Electronics Department of the University of Dundee for making the TIROS pictures available to me and it is with the University's kind permission that the photographs are reproduced.

## References

- |  |      |  |
|--|------|--|
| Geneva, World Meteorological Organization                | 1973 | The use of satellite pictures in weather analysis and forecasting. <i>Tech Note</i> No. 124.   |
| Scorer, R. S.  | 1972 | Clouds of the world. Newton Abbot, David & Charles.  |
| Washington, Air Weather Service, United States Air Force | 1969 | Application of meteorological satellite data in analysis and forecasting. <i>Tech Rep</i> 212. |

## Awards

### L.G. Groves Memorial Prizes and Awards

The annual award of prizes took place on Friday 30 November 1979 at the Main Building, Ministry of Defence, Whitehall (see Plates IX–XII). It was in one respect a sad occasion because since the previous prize-giving Major Groves himself had died; everyone present, however, was glad that the work he and Mrs Groves had initiated and inspired for so many years was being carried on and that his great-nephew, Mr Nicholas Abbott, would be presenting the prizes. The Vice Chief of the Air Staff, Air Marshal Sir John Nicholls, K.C.B., C.B.E., D.F.C., A.F.C., presided.

Sir John Nicholls, in his opening remarks, paid tribute to the late Major Groves and to Mrs Groves. He said how pleased he was that Mr Abbott was with them to make the presentations, and he was glad to see that so many other members of Major Groves's family had been able to attend.

Mr Abbott then read a message from Mrs Groves expressing her regret that owing to ill-health she was unable to be present. He referred to his long-standing family connections with the Royal Air Force, and said how honoured he felt to be presenting the prizes now that, sadly, his great-uncle was no longer there.

The 1979 Aircraft Safety Prize was awarded to Wing Commander H. W. Hughes, now serving with the Canadian Armed Forces in Winnipeg, for his proposal to improve the Canberra PR7 emergency warning system, with the following citation:

'Most aircraft in current RAF service are equipped with a standard warning system which monitors the important aircraft systems and presents the crew with an audible and visual warning when a failure occurs. However, because of their early design, Canberra aircraft do not have such equipment. Royal Air Force Canberra PR7 aircraft are employed primarily in the tactical reconnaissance role and the crew have to spend the majority of their time looking outside the cockpit for terrain avoidance, low-level map reading, target acquisition and reporting. Therefore, an airborne emergency could develop to a serious degree before the aircrew would be alerted by the warning lights in the cockpit. Wing Commander Hughes's proposed modification to remedy this flight safety hazard re-routes the engine fire warning, the engine fuel low-pressure warning and the generator warnings and connects them in parallel to the cabin pressure warning circuit, which is fitted with a warning horn. Thus, if one of these vital systems were to fail the crew would receive timely audio and visual warning enabling them to take action before the situation worsened.'

Wing Commander Hughes was unfortunately unable to be present to receive his prize in person.

The 1979 Meteorology Prize was awarded to Mr D. E. Miller of the Meteorological Office with the following citation:

'Mr D. E. Miller has been a pioneer in the application of satellites to meteorology. In recent years, as a consultant to the European Space Agency and member of various working groups, he has made many contributions to the development of the Meteosat system. In particular, he has been concerned with the design of the computer-based system which transforms the raw data from the satellite into usable pictures and other meteorological products. He has also been the leader of the team which has developed an instrument for measuring temperatures in the stratosphere from the TIROS-N series of satellites, the first two of which have been launched successfully in the past year.

'His deep understanding of the scientific problems, coupled with a sound appreciation of the need to obtain practically useful results and an energetic and painstaking attention to detail in overcoming difficulties, have made him an outstanding member of the teams working on these two projects and contributed greatly to their success'.

The 1979 Meteorological Observer's Award was awarded to Flight Lieutenant J. Holland, now at Royal Air Force Odiham, with the following citation:

'Flight Lieutenant Holland joined the Meteorological Research Flight (MRF) in August 1975 and was posted out in April 1979. During this time, as well as being a first-class navigator, he readily identified himself with the special requirements of MRF project flying and also made valuable contributions to the preparation of the various Trials Instructions. His calm approach in all aspects of the work was always a tower of strength, particularly so on projects such as the JASIN (Joint Air-Sea Interaction) experiment where several—sometimes foreign—aircraft were also involved within a limited airspace, and accurate navigation was at a premium.'

The 1979 Second Memorial Award was awarded to Flight Lieutenant J. S. Garnons Williams of Royal Air Force Shawbury with the following citation:

'The danger of a wirestrike is an ever present hazard in helicopter operations and military helicopters, which spend much of their time at low level, are particularly at risk. Moreover, we are likely to see an increase rather than a decrease in low-level helicopter operations in the future. While considerable effort has been devoted to solving the birdstrike problem for fixed-wing aircraft, little research has been devoted to the similarly hazardous and costly problem of helicopter wirestrikes. Flight Lieutenant Garnons Williams has proposed a simple wire-cutter that would protect the aircraft—apart from the main rotor—against all but the thickest power cables at speeds as low as 30 knots. The advent of wire-guided anti-tank missiles has added a new dimension to the helicopter pilot's problems since any future battle area will be festooned with fine wire. It is possible, therefore, that Flight Lieutenant Garnons Williams's idea may prove operationally valuable as well as reducing the peacetime cost of wirestrikes.'

551.5:358:92

## Memoirs of an Army Meteorologist

By H. Cotton, M.B.E., D.Sc.

### Part 6

A few days after the German push had failed and things had settled down again, we made another, and this time final, move. The new site was at Senlecques, a village, very pleasant after those we had been used to, with comfortable brick-built houses and a *place* quite imposing for the size. It was situated on a secondary road, leading west, from the main road through Desvres to Calais. So we were almost as far away from the war as we could be and to all intents and purposes we were doing a civilian job, there being no possibility of alarms of any kind: shelling, bombing, or strafing of the balloon by enemy planes—the great fear of kite balloonists because even if they jumped and the parachute opened, they were likely to be machine-gunned. To the enemy the idea of giving a helpless man a sporting chance was not on. For once Dinkle was delighted since both Calais and Boulogne were an easy run in the light tender and there would be plenty of girls in both places. And so it proved, and needless to say, he was now always short of money. What the reason was for our removal to a place so remote from the war areas I never knew but, needless to say, nobody grumbled.

When we came to Senlecques, such a quiet spot, I thought that there could be no possibility of alarms or excitements, but I forgot to take the weather into account. Kite balloons are fair-weather appliances and are not meant to ride out violent storms. After the collapse of the second German offensive there was a period of comparative quiet but, as there was now an almost unlimited supply of guns and ammunition, there was continuous harassment by shell-fire and this, to be effective, necessitated the supply of wind corrections. There was a long spell of stormy weather during which observations of anything above ground level were impossible. The Air Force was grounded and pilot balloons were whisked away as soon as they were released, hardly rising at all. With almost gale-force winds, low scudding clouds and frequent squally showers, the kite balloon was also grounded and so, for a considerable period there were no data of any kind, apart from the surface wind, from which to compute the information required by the artillery. We consequently had nothing to do as the weather was too bad even for walking.

One morning Lieut. West came to me with a face like thunder.

‘Wing says that we have to go up now. I told them that it would be suicidal but they said that it was an order from G.H.Q. You’d better ring up G.H.Q. and tell them that it is quite impossible.’

‘I can’t do that’, I said; ‘Wing is sure to have told them what conditions are like and, in any case, they have the reports of all the Meteor observers. Quite frankly, with my quite insignificant rank I am not prepared to question the orders of G.H.Q.’

‘You mean you are prepared to go up, even in this?’

‘I don’t see any alternative. After all, millions of men have died already in this war, so why should they bother about two unimportant subalterns? I don’t mind telling you I am scared stiff at the idea, but since G.H.Q. have said that we have got to go up, we have got to go up. You are in charge of the

balloon but remember, it has been used solely for the purposes of Meteor, and you must admit that we have had a cushy time. So come on, let's go and get it over. Tell Dinkle to say his prayers for once and pray that we don't break loose and land somewhere in Siberia.'

Dinkle expressed his disapproval in the most picturesque language and the balloon crew thought we were crazy, as undoubtedly we were. Once released from its mooring the balloon was almost unmanageable, the men on the handling guys being dragged backwards and forwards and the basket bumping violently on the ground. I was afraid that when they 'let go the guys'—I didn't laugh this time—a man might be too late and find himself carried into the air as had sometimes happened. Fortunately we were spared that calamity.

Once released from the ground the balloon was in the control of the wind and, at first, it wasn't too bad, but when we had risen about 100 feet it demonstrated what it could do. After all it was a kite, so in addition to the rolling, pitching and shuddering that one experiences on a small boat in a rough sea, it responded to sudden changes in wind velocity, plunging violently downwards when there was a lull and ascending equally violently when the velocity suddenly increased. Stanley Holloway could have written a comic monologue about 'this up and down kind of existence', but to us it was far from funny. The amplitude of these plunges increased the higher we rose and I was surprised to realize that I did not even feel seasick. No doubt it was because I was too scared.

With all the cable paid out by the winch, we reached only 1500 feet, the cable being almost horizontal apart from the sag in it. I had been watching and listening to the metal V by which the balloon was attached to the cable. It was on this that I had to take a compass bearing. I did not like its violent to and fro working for it was not unlike the way one bends an iron bar backwards and forwards in an attempt to break it.

'What do we do if that thing breaks?' I asked, trying to sound nonchalant and feeling anything but.

'What the hell can we do?'

'I know one thing,' I said; 'I am not going to risk jumping; what about you? But of course you can't leave the ship as long as I am here.'

It was obvious that jumping in a wind like that would be suicidal, so there was nothing for it but to stay where we were and hope for the best. Actually the worst was still to come. During the descent the up and down plunges were, if anything, worse than before, as the balloon thought up a new trick which it hadn't used during the ascent. After a little while, in addition to doing all its other tricks, it began to yaw from side to side, only a little at first but the amplitude gradually increasing as the distance between balloon and winch decreased. It soon became clear that the yawing, instead of being a simple oscillation in a straight line, was motion along the arc of a circle and that as the length of paid-out cable decreased the angular extent of the oscillation progressively increased. The sum total of all the motions to which the balloon was subject was impossible to deduce. I forgot all about my fear of breaking loose and, instead, wondered what antics the balloon would be up to when we nearly reached the ground. With only about 50 feet of cable paid out the motion due to yawing was almost a complete semicircle and, whatever we felt, we must have looked damned silly from the ground.

With about 30 feet of cable paid out the tips of the handling guys momentarily just touched the ground and then we immediately set off on one of our semicircular orbits in the opposite direction. It was fortunate that the winch was in the middle of the field so that there were no obstacles of any kind. Soon our basket was bumping on the ground and the balloon crew, Dinkle among them, running like mad from side to side trying to grasp the handling guys. I was afraid that a man might get hold, be afraid to let loose, and be dashed to the ground on the other side. Fortunately this did not happen and the balloon was finally brought under control. As we thankfully clambered out of the basket, Dinkle ran up and said, 'Come and have a drink'. It was, I think, the most sensible thing he ever said.

I did not think that the data I obtained could have been of much value for, apart from rising only 1500 feet, the kite effect prevented an accurate assessment of the height. Actually with a wind of that nature there is little change of either velocity or direction with height, so perhaps the data would have been of some limited use.

The ascent was, without doubt, a most hazardous undertaking and I agreed with West when he said we deserved a medal. Unfortunately there was no V.I.P. watching us.

When it was over I was glad that I had had the experience, but I would not like to have to do it again. An ascent in such a wind had never been made before and has never been made since. On this occasion, at any rate, my job had been far from cushy.

At 11 a.m. on 11 November, the Armistice was signed. The weather was unsettled over much of France, but at Senlecques it was fine, warm for the time of year, and the sun was shining. A message came from G.H.Q. to the effect that the balloon ascents were to continue, and I was glad, for to stay in such a quiet place, doing nothing, would have been intolerable. The two balloon officers were at first inclined to rebel but they quickly realized that with a body of men waiting to go home and with nothing to do, there would soon have been trouble.

In the middle of the morning I went for a long walk and tried to feel elated, but without success. The war had gone on so long, had become almost a way of life, that it was difficult to realize that it had ended at last. So, whereas for everybody else the war had ended, for us the war, such as it was, continued.

Until the Armistice conditions were fulfilled, the Army continued in a state of combat readiness since, until then, the war was not really ended. Finally, there was a general move forward and, to our regret, we had to move forward as well. Our route was in a general easterly direction, through St Omer, Poperinghe, Ypres, the dreadful landscape of Passchendaele, now shell-torn and pock-pitted but otherwise a featureless desert apart from the straight road running through it. The Passchendaele Ridge looked so insignificant geographically that it was difficult to realize that it could be the cause of such dreadful carnage, that this quagmire, at that time waterlogged by almost unending pitiless rain, was the last thing that would be seen by the legions of men, German and Allied, who died there, victims of forces they did not even understand.

At one place, by the roadside, there was a boot with a splintered shin bone, bleached white and clean as though it had been immersed in a zoologist's tank of tadpoles, pointing accusingly to Heaven as though in mute protest at the unspeakable things God had allowed men to do to one another. I wondered to what kind of man had this pitiful thing belonged.

Our destination was a rather drab little village on the road to Roulers but just west of that town. My billet was a little house in a row of little houses and the mess was in a larger house facing the main road. As we were merely marking time, I remember little about the place; a ruined church with smashed organ whose pipes looked like protruding intestines; sheets of music in archaic notation which today would probably be very valuable but were then scattered all over the place; an empty house with, scrawled on the wall,

*Ost oder West*

*Da Heim ist das best*

and this, which seemed to indicate a dumb acceptance of something too big for understanding,

*Lernen leiden ohne zu klagen*

unlike the British Tommy who also suffered but not with such bovine acceptance. It appeared that the Germans were human after all.

There was a pile of 76 mm shells just outside the mess window, the shell cases of sheet iron because of the shortage of non-ferrous metal owing to our naval blockade. After lunch one day, I was sitting

by the fire, reading. It was a cold, wet day and the 1 p.m. balloon ascent had been most unpleasant. I heard a banging going on just outside the window and when I investigated I found to my horror a small boy sitting on the pile of shells, banging away with a hammer trying to remove a nose cap. I chivvied the boy away and hurried to find Lieut. West, as being the O.C. it was his responsibility. He evidently contacted the right people for the shells were gone the next day. It would have been an irony if we had all been blown up after the war had ended, not by the Germans but by a young souvenir hunter.

We heard of the beginnings of discontent among troops who, now that the war was over, thought that they ought be shipped home straight away. There was no trouble with the balloon crew and I am sure it was because we were still making the three ascents a day.

Late in December I was notified that I was to be demobilized and I received the necessary papers enabling me to return first of all to Montreuil, the G.H.Q. personnel having remained there while practically everyone went east. Very early on a bitterly cold and wet morning I was taken by tender to Roulers station and after an interminable wait, during which I became colder and colder, the train crawled in. I had what had been a first-class compartment all to myself and, if it had been possible, I would have joined a compartment full of troops, if only for the warmth. The windows were broken and the upholstery in shreds; in fact it would have been a disgrace to the then Great Eastern third-class commuter carriages, and that is saying something. I left the train very thankfully at some place whose name I have now forgotten and made my way to what, I think, must have been a hotel before the war. After about 14 hours in a perishingly cold train, with nothing to eat, I was almost overwhelmed by the warmth and it was some time before I became sufficiently accustomed to it to face dinner. Eventually I did, and very welcome it was. I am quite sure it was only the outdoor life of the last three years which enabled me to survive that journey without illness.

The train journey to Montreuil was much more comfortable. For one thing I had eaten a very good breakfast, and for another I found a compartment with all the windows intact and several passengers all puffing warmth from cigarettes and pipes. It was a joy to get back to the civilized G.H.Q. mess. Life at Meteor went on exactly as before; the war may have ended but the weather was always with us.

Early in January I said goodbye to everyone at Meteor, regretfully for, being one of the very lucky ones, because of the war I had profited both physically, owing to the open-air life, and intellectually. From an early age I have taken a delight in the processes of nature, those I could understand and those which, as yet, I could not understand. Without a doubt there is a strong streak of pantheism in my philosophical make-up. And so I fitted almost automatically into the organization of Meteor. By day and by night I had watched and recorded the ever-changing pageant of the sky, the magnitudes and kinds of the clouds from the filaments and frills of cirrus to the towering masses of cumulus reaching up into the sky and spreading out horizontally to form the thunderheads, precursors of the storm. Occasionally there would be a solar halo, and at night there was the occasional coloured lunar corona or the thin white circle of the lunar halo caused by reflection—not refraction—from minute ice crystals.

But, in spite of so much beauty, there was an unpleasant dichotomy which I could not always relegate to the back of my mind. The work of Meteor was not carried out to further the science of Meteorology but to make more accurate the processes of mass slaughter. When I was recording upper-air phenomena and computing the artillery wind corrections, I was, in a sense, serving with the artillery. After the war had changed its character from one of movement to one of the stalemate of the trenches, it changed from an infantryman's war to an artilleryman's war. As Hogg and Thurston point out in *British Artillery Weapons 1914/18*, there were operations in which more gunners were engaged than assaulting infantry. And yet, unless accurately directed, a bombardment could result in an unacceptable waste of ammunition. A vital factor affecting accuracy was the reliability of the wind corrections, and

it is no exaggeration to claim that the value of the artillery was, in large measure, dependent on the work of a handful of men, mostly of non-commissioned rank.

Thus I left Meteor with mixed feelings of gladness and of sorrow, and I marched down the hill to the railway station and to my strangest experience of all. I found that the most important function of a subaltern was to escort parties of men to those dreary places called rest camps. So, immediately my train arrived at Boulogne, before it had stopped even, every officer below the rank of captain dashed out and tried to find somewhere to hide; under the carriage, between the buffers, in the far corners of those enormous wagons labelled *hommes quarante chevaux six*. But it was not the slightest use, for by now the M.P.s knew all the tricks and I am quite sure they would have winkled out a man trying to hide in the smoke-box of the engine. Knowing the futility of these attempts to escape, I took my time and stepped out of the carriage straight into the welcoming arms of an M.P. I was conducted to a party of men at the foot of the hill leading to the rest camp. Fortunately for me they were already formed up in fours, so all I had to do was wait until the lot in front of me moved off, wave my arm, shout 'Come on', and follow up the hill.

Rest camp! That name must have been thought up by the G.H.Q. chief comedian. It was just about the bleakest place I had encountered, at the top of a bare hill and raked by wind from every quarter. What it must have been like during a blizzard, I couldn't imagine, and what the rest camp must have been like for the other ranks I couldn't imagine either. Everyone should have been delighted at the prospect of going home, this time for good, but there was precious little sign of happiness even among the P.B.I. who surely had the most cause for rejoicing.

After breakfast the fun started. A corporal conducted me to my bit of the army, two long rows of men, numbered them off, saluted and marched away. I looked at them and I hadn't the foggiest idea what to do. Everybody else's war was ended but mine was just starting. There was only one thing to do, and that was watch the party which would march off before mine and listen to the commands. The officer in charge had a loud voice and the command was: 'Move to the right in fours; form fours. Left turn. By your right, quick march'. I could easily memorize that for the time required but, unfortunately for me, I missed something very important. So in my best barrack-square voice I gave the command, 'Move to the right in fours'.

But they didn't move to the right, or to the left. They just looked at me and I looked at them. So I tried again but with the same negative result. Something was very wrong and I had no idea what it was. Being swallowed up by the providential opening up of the earth was not provided by an indifferent Nature, so there was only one thing to do. *Toujours l'audace* once again. I didn't know what to do but they did. So they should tell me and I would make it look as though it was their fault.

'What the bloody hell's the matter with you', I shouted, 'Don't you want to go home?'

That did the trick; a man in the rear rank called out, 'Please sir, you didn't say form fours'. So that was it, I ought to have given the order, 'Move to the right in fours; form fours'.

By this time I was thoroughly browned off by the ludicrous nature of the situation. If I tried again I would remember 'form fours' but possibly forget something else. So I shouted, 'O bloody hell, come on'.

I waved my arm, stalked off the battlefield and left my bit of the army to sort themselves out as best they could and follow me down to the docks. There were some brass-hats present but what they thought of the farce I had no idea, and didn't care either.

As though this fiasco wasn't enough there had to be another ludicrous incident on board ship. After seeing all the men on board, I decided that I never wished to see them again, so I found a corner on the boat where I fondly believed that I should remain undiscovered. I had not realized that if someone gives you a bit of the army the men will stick to you like a long-lost brother. Also, I did not know



that another of the subalterns' duties was to act as a nursemaid and sort out all the men's troubles.

It was a cold day with strong winds and heavy showers and it was obvious that we were in for a rough crossing. We had been at sea for half an hour or so when one of the men found me. His feet were bare and he carried trench boots, one in each hand. By this time nothing could surprise me, so I waited for his explanation. I expected that he had probably scrounged the boots and then, too late, found them too uncomfortable to wear. His trouble was that somebody had been seasick in one of them, and what could he do. I hadn't the least idea. Meteor had not taught me how to deal with a situation of this kind, and I forget what advice, if any, I proffered. I expect I had very little sympathy as I was still smarting from the comedy of 'form fours'.

So for me the most dreadful war in history ended on a note of farce.

*(Concluded)*

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The complete text of Dr Cotton's memoirs is available in the National Meteorological Library, Bracknell, and in the Imperial War Museum.

We print in this issue (see Plate XIII) a copy of a synoptic chart and weather report prepared by Dr Cotton while he was a member of Meteor; he presented this chart to the National Meteorological Library a few years ago.

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## **World Meteorological Organization Commission for Agricultural Meteorology (CAGM) Seventh Session, Sofia, September 1979**

By W. R. Sparks

(Meteorological Office, Ministry of Agriculture, Fisheries and Food, Bristol)

### **Introduction**

The seventh session of the WMO Commission for Agricultural Meteorology was held in the Conference Room of the Hotel Europa, Sofia, Bulgaria from 17 to 28 September 1979. Representatives of 53 countries, four international organizations and three non-governmental organizations took part in the plenary meetings; the United Kingdom delegates were Messrs C. V. Smith and W. R. Sparks. The session was opened with a speech by the Bulgarian Deputy Minister of Agriculture, Mr Ivan Tonev, and on the opening morning the Commission was also addressed by Academician C. Daskalov on behalf of the Bulgarian Academy of Sciences, Mr R. Schneider, Deputy Secretary General of WMO, Mr G. Popov, representing the Secretary of the United Nations Food and Agriculture Organization, Dr I. M. Nur, representative of the Organization of African Unity and finally by the President of the Commission, Dr W. Baier.

These opening speeches were not merely bland addresses of welcome to the delegates; they set the background against which the Commission must work. The President pointed out that during the 10-year period ending in 1975, world wheat production rose by 50 per cent, maize production by 38 per cent, sorghum by 35 per cent and rice production by 30 per cent; at the same time, world population increased by about 30 per cent. In order to increase food production in the 1980s at the same rate as the expected increase in population it will be necessary to continue to:

(1) Expand crop areas into present forests and pasture lands, into colder areas, drier areas, wetter areas and areas with problem soils.

(2) Increase yields on existing farmlands.

(3) Change farming systems so that more crops can be grown within a 12-month period on the same land. Such a policy of agricultural expansion is not without dangers. Already vast areas of once productive land have been lost and are still being lost by desertification and the Commission were reminded by Dr Nur that among the practices that can result in desertification are:

(a) Overgrazing.

(b) Cultivation of marginal land.

(c) Deforestation.

It is an uncomfortable fact that in many dry areas the intensity of agriculture that maximizes production in the average year can lead to desertification during a dry period. Similarly, in wet areas, agricultural practices that are profitable in most years may allow severe soil erosion during wet periods. There may be only a narrow path to tread between the production of adequate food for the population of the world and the destruction of the land from which that food must be produced.

With these thoughts in mind the Commission set about its three main tasks for the session:

(1) To review the progress since the previous session in 1974.

(2) To identify agricultural problems related to weather and climate.

(3) To propose a program of work and to appoint officers, working groups and rapporteurs to carry it out until the next session.

## Review of progress

At its sixth session the Commission had established nine working groups and appointed 14 rapporteurs, but the ability of the seventh session to review their work was severely restricted by the absence of full reports. Many of the reports were available to the Secretariat but financial restrictions prevented their reproduction and distribution to delegates. CAgM-VI had set up two particularly ambitious working groups to carry out international experiments for the acquisition of weather data related to the cultivation of lucerne and wheat. The potential value of such international experiments is great because in a few years they allow a cultivar to be exposed to a range of weather conditions far greater than it would experience in most individual countries in a century. But the control of non-meteorological factors and the quality control of the data proved very difficult. Both working groups managed to complete experimental programs but since the data have not yet been analysed it was impossible to judge the success of the programs.

Lack of information made it impossible fully to assess the work of the Commission; nevertheless, it is pleasing to report that during the interval between the sixth and seventh sessions of the Commission, 11 WMO *Technical Notes*, three CAgM information reports and a book on 'Agricultural meteorology' were published. A number of other reports, including the revised 'Guide to agricultural meteorological practices', are in the course of publication.

### **Identification of major weather-related problems**

The Commission had no difficulty in identifying problems that confront them; indeed the most important problems have been highlighted at major international meetings in recent years.

The World Food Conference explicitly called upon WMO to encourage the installation of observation and telecommunication networks in order that meteorological and climatological data could be placed at the disposal of the national authorities responsible for agriculture.

The United Nations conference on desertification called on WMO to support its plan of action. The World Climate Conference unanimously designated agriculture as one of the high-priority areas for work within the Climate Applications Program and the Climate Impact Study Program.

### **Proposed program of work**

Between sessions the Commission carries on its work through its President and his Advisory Working Group, and also through specialized working groups and rapporteurs. The Commission responded to the request by the World Food Conference by setting up working groups on 'Data requirements for agriculture' and 'The agricultural services in developing countries', and appointing rapporteurs on 'Agroclimatic maps', 'Drought probability maps' and 'Wild land fires'. The Commission's main support for the WMO plan of action against desertification will be through the working group on 'Agro-meteorological aspects of land management in arid and semi-arid areas' whilst the main contribution to the World Climate Program will be through working groups on 'Impact of climate variability on agriculture and agricultural activities on climate', 'The role of forests in the global balances of carbon dioxide, water and energy' and 'Meteorological aspects of agriculture in humid and sub-humid tropical areas'. In support of this program the Commission also appointed rapporteurs on 'Soil water studies' and 'Air pollution and plant injury'.

Much of the work described above is dependent on knowledge of the relationships between weather and crop and animal production. The Commission therefore planned to supplement its work on the major problems by continuing the analysis of the weather data for the lucerne- and wheat-cultivation and establishing working groups on 'Weather and animal health' and 'The effects of meteorological factors on maize development and yield'. It also appointed a rapporteur on 'The application of models for forecasting development and ripening of crops'.

In order to rationalize the WMO calendar of meetings CAgM-VIII will be held late in 1982 so that the Commission has only three years under its new President, Dr N. Gerbier of France, and its new Vice-President, Dr J. J. Burgos of Argentina, to complete the program of work it has set for itself.

### **Impressions of the session**

In addition to the factual report of the session the impressions of a delegate attending a meeting of the Commission for the first time may be of interest.

It was clear that the great majority of delegates were convinced that agricultural meteorology has a vital role to play if the world's food supply is to be expanded to keep pace with the increasing population without at the same time placing the productive capacity of the land at risk. The problems were discussed on a global or regional basis and national and political prejudices showed only rarely. The problems of developing countries received a great deal of attention and delegates from many of those countries contributed fully to the discussions. Among the African delegates, those from the ex-British colonies were particularly active and their representation on the newly established working groups of

the Commission is far greater than that of the United Kingdom, which is represented on only two of the working groups. This level of contribution from the United Kingdom was not due to our reluctance to volunteer names for working groups but to the large number of candidates put forward by other delegations, and the nomination and selection procedures adopted by the Commission. It should also be said that although our present involvement with the work of the Commission is slight compared with our own past standards, it is comparable with that of all other western European countries with the exception of France, which now has the Presidency.

A meeting of a WMO Commission relies very heavily on the services of the WMO Secretariat to keep it on schedule and, although there were times when I felt that I was being beaten into submission by the sheer weight of paper heaped upon me, I finished the fortnight with a considerable respect for the efficiency and patience of the translators and the WMO staff whose work was carried out under less than ideal conditions.

I found the experience of attending a session of CAgM exhausting but interesting. Particularly rewarding were informal discussions with agricultural meteorologists from other countries about the services they provide and the opportunities and frustrations of their work. It appears that the resources devoted by the United Kingdom to agricultural meteorology are small compared with those of many other countries and it is clear that we must use our resources with great efficiency if we are to maintain our place among the leaders in the provision of meteorological services to agriculture.

### Review

*Pathways of pollutants in the atmosphere (A Royal Society discussion organized by T. M. Sugden, F.R.S. for the Royal Society's Study Group on Pollution in the Atmosphere, held on 3 and 4 November 1977).*

The Royal Society of London. 290 mm × 210 mm, pp. vi + 169, *illus.* The Royal Society, 6 Carlton House Terrace, London SW1Y 5AG, 1979. Price £12.90 (United Kingdom addresses, including packing and postage) and £13.30 (overseas addresses, including packing and postage).

This volume contains papers presented at a Royal Society Discussion Meeting organized as part of the program of work of its Study Group on Pollution in the Atmosphere and first published in *Philosophical Transactions of the Royal Society of London*, Series A, Volume 290 (No. 1376), pages 467–637. It comprises twelve major invited contributions by leading experts in different branches of the subject followed in most cases by short reports of the ensuing discussion and, as would be expected, provides an authoritative and up-to-date review of the present state of knowledge in this area and the philosophies of future monitoring and control of atmospheric pollution.

The pathways through the atmosphere of materials from their emission regions or 'sources' to their removal regions or 'sinks' involve transport and dispersal by the atmospheric motions during which changes due to chemical and physical processes generally occur. The pollution problems may be essentially global, regional or local and consequently their study may require meteorological information extending from the boundary layer to the stratosphere or above as well as appropriate data on the atmospheric physics and chemistry. In addition to establishing the exposure of all the 'targets' or receptors of the pollutants (e.g. humans, animals, plants, ecological systems, human artefacts and indirectly, climate) it is necessary to produce reliable dose-effect data which involve many different scientific disciplines. Given the overall scientific basis linking the sources to the final effects it is then necessary to seek, taking into account the existence of natural sources, the best policies for the control and monitoring of the pollution in the future.

The background to the current problems is reviewed by T. M. Sugden in the Preface and in the first paper 'The classification of pollutants and their pathways in the atmosphere'. In this he identifies the trends which have directed increased attention to atmospheric pollution in recent years, namely the impact of new and increased technology and the growing social awareness of its possible hazards. This has resulted in this country *inter alia* in an acceleration of governmental studies on pollution controls, a recent Royal Commission Report, and detailed studies by the Royal Society Study Group on a wide variety of pollution topics. The last include (i) possible global effects arising from stratospheric aircraft flights, increased use of nitrogenous fertilizers and fluorocarbon releases on stratospheric ozone and also possible climatic changes following increases of carbon dioxide in the atmosphere (ii) regional effects produced by the transport of sulphur compounds in north-west Europe (iii) local effects due to airborne heavy metals and (iv) the philosophy and practice of air pollution monitoring. Transient pollution due to accidental releases of toxic substances has also been considered.

Papers relevant to the problem of possible decreases in stratospheric ozone and the consequent increase of potentially dangerous ultra-violet radiation at the earth's surface include a review of the complex photochemistry involved together with recent results from laboratory studies by B. A. Thrush, an investigation of the current atmospheric modelling techniques used in the assessment of possible ozone reduction by A. F. Tuck and a comprehensive review of atmospheric nitrous oxide (considered to be the main source of nitrogen oxides in the stratosphere) by J. Hahn. All of these papers present recent work and are of great interest to specialists and other workers in the area. It would have been more helpful to the general reader of this book (as contrasted with the usual reader of the Royal Society's *Philosophical Transactions*) if more background and possibly some historical information on this overall problem had been included somewhere in this group. In contrast the following paper, a review by J. T. Houghton of possible climatic changes arising as a result of the 'greenhouse effect' due to an increase of carbon dioxide from the burning of fossil fuels or the release of chlorofluorocarbons in the atmosphere, is self-contained and a useful introduction for any scientists interested in atmospheric processes.

The regional problem as exemplified by the transport, dispersion and deposition of sulphur pollutants over western Europe is well covered in a very informative article (which also reports studies initiated by the Organization for Economic Co-operation and Development) by F. B. Smith and R. D. Hunt. This is followed by a detailed discussion by R. A. Cox of the photochemical oxidation of atmospheric sulphur dioxide.

In local pollution problems the contribution of meteorologists is mainly through assessment of short-range dispersion by winds in the boundary layer. This approach is used together with measurements of particle sizes and concentrations in an important study by A. C. Chamberlain and his colleagues of the dispersion of lead from motor exhausts. Another comprehensive and specialized paper on atmospheric chemistry relating to conditions near the surface is given by J. N. Pitts Jr whose subject concerns types of 'non-criteria pollutants' (the nomenclature in the U.S.A. for pollutants other than the well-known 'criteria pollutants' sulphur dioxide, carbon monoxide, nitrogen dioxide, photochemical oxidant (including ozone) and total suspended particulates) and is entitled 'Photochemical and biological implications of the atmospheric reactions of amines and benzo(a)pyrene'.

Questions of the factors determining the amount of damage that is likely to be caused to a receptor by a given dose of pollutant (the product of concentration and time of exposure) are discussed by M. W. Holdgate. These include variations in target sensitivity and the effects of single and multiple impacts and the author concludes that there is a need for an integrated approach including epidemiological studies, controlled experiments relating dose to effect and lastly models to provide a framework for interpreting the system as a whole. The subject of monitoring for explanation, observation and

control is then discussed in some detail by J. S. Reay with the important reminder that monitoring is not to be confused with mindless measurement. Finally the philosophy of control of air pollution in the United Kingdom which is based on the 'best practicable means' concept, in which it is recognized that a degree of air pollution is inevitable in present-day industrial society, and strives to optimize this pollution so that the total interest of society is best served, is expounded by F. E. Ireland and D. J. Bryce.

In summary, this collection of papers is clearly of great importance and provides a very useful reference to the present overall state of the subject both as regards scientific progress and its application to the practical problems involved. This presumably is the reason why the decision was taken to reproduce these *Philosophical Transactions* as a book easily accessible in libraries to a more general readership. At the same time it should be noted that the coverage is not uniform, some of the articles being quite easily read and understood by scientists and others on the fringe of the subject while some of the papers, although of great importance in their own right, can only be fully appreciated by specialists in the relevant field. It would have been helpful if the Royal Society document 'Pollution in the Atmosphere; Final Report of a Royal Society Study Group' published in June 1978 could also have been incorporated in this book to provide additional background and the interested reader is also advised to obtain this document.

R. J. Murgatroyd

### Obituary

We regret to record the death on 26 October 1979 of Mr J. A. Lynch, Higher Scientific Officer. Mr Lynch first joined the Office as an Assistant in 1941. After service with the RAF Met. Branch, he was reinstated as a civilian in 1947, and from 1950 to 1959 he occupied a Senior Assistant post at ATCC Uxbridge. In March 1959 he was posted to the meteorological equipment (stores) section of M.O.4 in Harrow, and in this post became known to many members of the Office; his hard work made a valuable contribution to the smooth functioning of Headquarters branches and outstations alike. He was promoted to Higher Scientific Officer in 1978.



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## NOTICES

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Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd, 24-28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 586891 K15 3/80

Annual subscription £20.82 including postage  
ISBN 0 11 722059 0  
ISSN 0026-1149



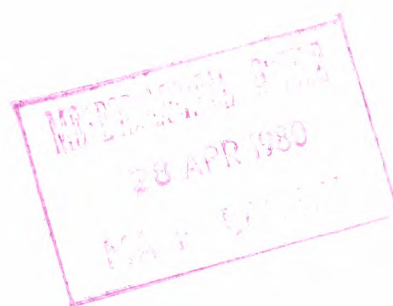


# THE METEOROLOGICAL MAGAZINE

HER MAJESTY'S  
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OFFICE

April 1980

Met.O. 931 No. 1293 Vol. 109



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# THE METEOROLOGICAL MAGAZINE

No. 1293, April 1980, Vol. 109

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## Statistical comparison of central England annual and monthly mean air temperature variability, 1660–1977\*

By C. D. Schönwiese  
(Munich)

### Summary

A series of surface atmospheric temperature values in central England from 1660 to 1977 is used to compare the variability of the annual mean with that of the individual monthly means. Firstly, correlation analysis of yearly and monthly data yields a mean correlation coefficient of  $+0.45$ . On investigating the correlation with a Gaussian low-pass filter for a period  $T > 30$  a (where 'a' denotes 'year') it is found that the mean correlation coefficient increases to  $+0.65$ . Significant ( $> 99$  per cent) maxima of correlation occur in spring (March,  $+0.89$ ) and autumn (October,  $+0.81$ ), and significant minima in winter (February,  $+0.50$ ) and summer (June,  $+0.52$ ).

The low-pass filtered annual and monthly data display remarkable agreement with respect to the increase in temperature from 1694 to 1731/32. Otherwise, however, very clear differences are to be seen, including the increase in temperature in the first half of the twentieth century. The most recent relative maximum in annual values was reached in 1946, whereas the May values peaked as early as 1917 and the October values not until 1964. Finally, comparison of the variance spectra of the annual and monthly data indicates similarities in the range of relatively long periods  $T > 50$  a, and in connection with quasi-biennial fluctuations.

### Introduction

Long-term series of meteorological measurements (see, for example, Lamb 1972, 1977; v. Rudloff 1967; Schönwiese 1974) reflect the variability of our climate. Whereas the year-to-year fluctuations mostly present a very confusing picture, it is possible, with the aid of statistical methods, to codify this variability in terms of its components and to represent it in summary fashion.

Thus, low-pass filters (Mitchell 1966; Schönwiese 1978d; Taubenheim 1969) have the effect of showing up long-term trends and largely suppressing the components of fluctuation which have a relatively short period, for example  $T < 10$  a or  $T < 30$  a (where 'a' denotes 'year'). Interstation comparison of this type of filtered data makes it possible to consider parallels and phase shifts in long-term fluctuations, something which the direct year-to-year variation reveals either not at all or only very dimly. Such an investigation has recently been published (Schönwiese 1978d) for some temperature series in Europe and for the series in the northern hemisphere.

Apart from the long-term fluctuations—regular or irregular—there is a further aspect of particular interest: how is the variability of a series of measurements distributed throughout the range of periods,

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\* Translated by G. Spence from *Meteorol Rundsch*, Berlin, 32, 1979, 73–81.

and are there signs of cyclic variability? The questions are answered by means of spectral variance analysis (Mitchell 1966; Panofsky and Brier 1958; Schönwiese 1969; Walk 1970) and numerical band-pass filtering (Brier 1961; Mitchell 1966; Schönwiese 1974, 1975).

The variance spectrum gives the distribution of the variance in relation to definite intervals of period and frequency. By means of numerical band-pass filtering it is possible to consider the variability of the time series in isolation within more or less narrowly limited intervals of period and frequency. Cospectrum and cross-spectrum analysis may be used, in conjunction with coherence analysis (Doberitz 1968; Fleer 1975; Walk 1970) to compare the variability of two time series, also in relation to period or frequency.

If consideration is limited to the range of periods  $T > 1$  a—the ‘climatological scale’ (Schönwiese 1978c)—and to neoclimatological series, that is to say, those based on direct measurements, it is only in a few instances that detailed discussion can be undertaken for a span of several hundred years. The longest closed and homogeneous climatological time series available in the form of monthly mean values is the series of surface air temperatures for central England (Manley 1974), which begins in 1659. Statistical analyses of the variability observed in this series are already to hand (Manley 1974; Schönwiese 1978a, 1978b, 1978d), and serve different goals.

In relation to the long-term ( $T > 30$  a) component of annual data fluctuation there are relative maxima in 1732, 1828, 1867, and 1946, and relative minima in 1694, 1813, 1840 and 1887; see the continuous curve in the first part of Figure 1, and compare also Schönwiese 1978d. For the variance as related to frequency, significant (the word implying here values above the 80 per cent confidence limit) maxima occur in connection with periods of approximately 100 a, approximately 25 a, 5.1 a, 3.4 a, 3.1 a and 2.2 a, and less significant maxima in connection with periods of 14 a and 7.4 a; see Figure 2, upper curve, and compare also Schönwiese 1978a. In the following sections these data are more closely analysed and discussed.

Before proceeding, however, one more consideration of general interest: statistical analyses, the methods of which are partly discussed here, are useful not only to provide an exact description of the data from various points of view. They ought also not to be underestimated in the interpretation of observations, in the search for the causes of variability, and in the problem of the predictability of the latter. They may also help to provide bearings for the climatic models in relation to the phenomena to be reproduced. Moreover, statistics are increasingly being incorporated directly into the models, particularly where success is lacking in the only partly applicable three-dimensional models of the general circulation.

### Connection between the annual and monthly means

The following question will now be examined within the framework of neoclimatology and the range of periods  $T > 1$  a: how far is the year-to-year variability influenced by the variability of the individual months? In an alternative formulation: over periods of a few or many years is it possible to find interannual trends in the means of individual months, as can be done for the annual means?—or are the long-term trends of annual means the result of a wholly irregular interplay of the monthly trends, and thus to be regarded as random?

These questions are of essential importance in the search for the causes of long-term fluctuations and in the discussion of their predictability, because if a wholly irregular picture emerges the fluctuations taking place must be regarded as the product of internal stochastic fluctuations in the climatic system and in the general circulation of the atmosphere; if not, non-random processes, including external causes, come into consideration as well (Mitchell 1976; US Committee for the GARP, 1975).

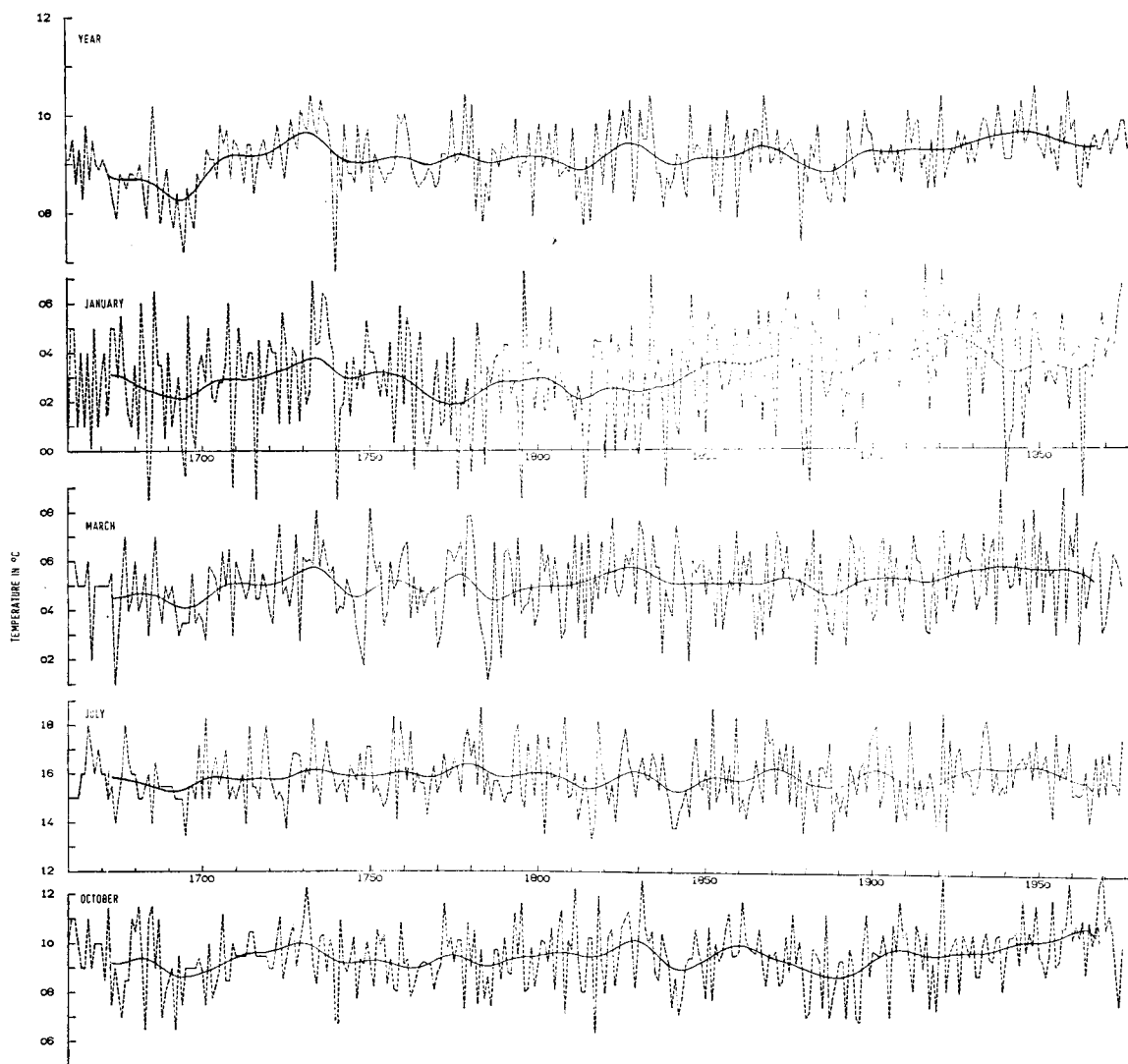


Figure 1. Mean air temperature in central England for the year as a whole and the individual months of January, March, July and October over the periods 1660–1977 (for the annual means) and 1660–1976 (for the monthly means). The dashed curves represent unfiltered values, the continuous curves values filtered with a Gaussian low-pass filter for  $T > 30$  a.



An attempt will now be made to answer these questions using the example of the temperature series for central England already mentioned. Table I gives the Pearson linear correlation coefficients (Haseloff and Hoffmann 1970; Sachs 1974) and their 90 per cent confidence intervals (Haseloff and Hoffmann 1970), as derived by comparing the annual means with means of the individual months over the period from 1660 to 1969.

It will be seen that the annual means are correlated with the means of all the months. The mean correlation coefficient amounts to  $+0.45$ , and the confidence intervals fluctuate between  $\pm 0.06$  and  $\pm 0.08$ . The differences with respect to the individual months are not very large: February has the maximum value, at  $+0.56$ , May the minimum, at  $+0.35$ . A 'double wave' is in evidence, with a relatively high correlation between the annual values, on the one hand, and the late winter and high summer values, on the other, and with a relatively low correlation between the annual values and the late spring and early winter values. However, these differences are significant at the 90 per cent level only from winter to spring (cf. the confidence intervals).

Certainly, a *t*-test of the correlation coefficients (Sachs 1974) indicates that even in connection with the smallest correlation coefficient (May,  $+0.35$ ) the significance of the correlation in itself is greater than 99.9 per cent. It should not be overlooked, however, that the computed coefficients lie in a position approximately midway between an ideal connection and no connection at all and, if anything, do not quite reach this position. Consequently, an approximate balance is held by parallel and opposing trends. In this connection the reader should see the dashed curves in Figure 1, in which, as an example, the means for January, March, July and October are represented in comparison with the annual means.

The next step is to follow the procedure detailed in Schönwiese 1978d for the purpose of applying a Gaussian low-pass filter (Mitchell 1966; Schönwiese 1978d; Taubenheim 1969) to all the values in such a way that all components of fluctuation with  $T < 30$  a are largely suppressed. It should be mentioned here that because of their much more favourable filter response function Gaussian low-pass filters and similar methods are much to be preferred to moving averages which themselves also operate as low-pass filters. Because of the secondary maxima of the filter response function which arise in the process of taking running means, it is impossible to avoid distorting effects, particularly when making step-wise multiple applications.

**Table I.** *Correlation coefficients between the annual and monthly means of surface air temperature in central England calculated over the period from 1660 to 1969.*

	Unfiltered values	Filtered values
Year — January	$+0.47 \pm 0.07$	$+0.54 \pm 0.07$
Year — February	$+0.56 \pm 0.06$	$+0.50 \pm 0.07$
Year — March	$+0.55 \pm 0.06$	$+0.89 \pm 0.02$
Year — April	$+0.48 \pm 0.07$	$+0.78 \pm 0.04$
Year — May	$+0.35 \pm 0.08$	$+0.60 \pm 0.06$
Year — June	$+0.41 \pm 0.08$	$+0.52 \pm 0.07$
Year — July	$+0.45 \pm 0.08$	$+0.60 \pm 0.06$
Year — August	$+0.48 \pm 0.07$	$+0.56 \pm 0.07$
Year — September	$+0.43 \pm 0.08$	$+0.75 \pm 0.04$
Year — October	$+0.45 \pm 0.08$	$+0.81 \pm 0.03$
Year — November	$+0.38 \pm 0.08$	$+0.66 \pm 0.06$
Year — December	$+0.37 \pm 0.08$	$+0.63 \pm 0.06$
Arithmetic mean	$+0.45$	$+0.65$
Standard deviation	$\pm 0.07$	$\pm 0.13$

*Note.* Concerning the filtered values, a Gaussian low-pass filter was applied which largely suppressed all components of fluctuation with  $T < 30$  a. The 90 per cent confidence intervals are shown next to the correlation coefficients.

The result of the correlation analysis of the values treated with a Gaussian low-pass filter to suppress periods having  $T < 30$  a is as follows: there is a significant increase in the correlation coefficients, the mean being  $+0.45$  for the unfiltered values and  $+0.65$  for the filtered ones. The 90 per cent confidence intervals of the correlation coefficients of the filtered data fluctuate between  $\pm 0.02$  and  $\pm 0.07$ .

The differences between the correlation coefficients are much higher for the filtered than for the unfiltered values, the standard deviations of the coefficients being  $\pm 0.13$  as against  $\pm 0.07$ . It is surprising that the smallest correlation coefficient of the filtered values, namely  $+0.50$ , for February, coincides with the highest coefficient of the unfiltered values,  $+0.56$ , also in February.

However, the 'double wave' of the correlation coefficients for the unfiltered data is less pronounced than that for the filtered data; more important, it is much less significant statistically. The correlation maxima of the filtered values (March,  $+0.89$  and October,  $+0.81$ ), and the corresponding minima (February,  $+0.50$  and June,  $+0.52$ ) remain significant if the confidence interval is increased from 90 to 99 per cent. Consequently, the differences between correlation coefficients must be seen as more important in connection with the filtered values.

It is conceivable that the high correlation between the annual values and the early spring and autumn data is connected through a feedback mechanism with the snow cover, which is particularly variable at those times of year, and with its albedo, in such a way that the occurrence or non-occurrence of a snow cover in these seasons has a particularly strong influence on the annual values. Interestingly enough, however, this holds only for the part of the spectrum of temperature fluctuation where the periods are relatively long, here  $T > 30$  a.

It is obvious from a careful interpretation of the results that in the band where periods are relatively long—and it should be noted that the limit  $T > 30$  a is, of course, arbitrary and in need of fuller explanation—the stochastic component of fluctuation is smaller than in the band of short periods. Consequently, it may be hoped that investigation of the appropriate long-term trends will lead more quickly to knowledge of systematic, that is to say non-stochastic, processes and their causes.

### **The comparison of long-term trends**

It seems reasonable, therefore, to look more closely at the long-term trends. This will be done with the aid, once again, of the temperature series for central England, modified by the use of a Gaussian low-pass filter of the type mentioned. The resultant annual and selected monthly values are shown by the continuous curves in Figure 1.

In connection with the annual values the following trends are particularly striking: the rise from the relative minimum of 1694 to the relative maximum of 1732, which according to Lamb (1972, 1977) is synonymous with the end of the culmination of the 'little ice-age' (cf. also Schönwiese 1978d); the less pronounced fall to the relative minimum of 1813; further, the rise from the relative minimum of 1887 to the most recent relative climatic optimum (Schönwiese 1978d) corresponding to the relative maximum of 1946, where this rise, according to Flohn (1978), is to be related to the end of the 'little ice-age'. Between these lie some less striking trends, namely the rise of 1813–28 and the subsequent fall as far as 1840, which was followed by a rise up to 1867, and, finally the fall of 1867–87.

An attempt will now be made to investigate whether the particularly pronounced trends of 1694–1732 (a rise of about  $1.4^\circ\text{C}$ ), 1732–1813 (a fall of about  $0.8^\circ\text{C}$ ), and 1887–1946 (a rise of about  $0.8^\circ\text{C}$ ) in the annual values are reflected in the corresponding monthly values. An answer may be found in Table II, which shows a noteworthy agreement between the monthly values and the relative minimum of the annual values in 1694. Certainly, calculations yield an arithmetic mean of 1690 for the year of



**Table II.** Comparison of the long-term trends in annual and monthly mean surface air temperatures in central England as derived by applying a Gaussian low-pass filter to suppress all components of fluctuation with  $T < 30$  a.

Year	Year of relative		$\Delta t$	Year of relative		$\Delta t$	Year of relative		$\Delta t$
	min.	max.		min.	max.		min.	max.	
Year	1694	1732	9.65 — 8.26 = 1.39	1732	1813	9.65 — 8.83 = 0.82	1887	1946	9.65 — 8.83 = 0.82
January	1694	1734	3.77 — 2.13 = 1.64	1734	1774	3.77 — 1.88 = 1.89	1890	1923	4.60 — 3.09 = 1.51
February	1681	1733	4.30 — 2.65 = 1.65	1733	1749	4.30 — 3.40 = 0.90	1891	1917	4.48 — 3.62 = 0.86
March	1695	1733	5.80 — 4.12 = 1.68	1733	1785	5.80 — 4.53 = 1.27	1887	1939	5.96 — 4.74 = 1.22
April	1696	1732	8.40 — 6.74 = 1.66	1732	1746	8.40 — 7.10 = 1.30	1886	1945	8.93 — 7.42 = 1.51
May	1694	1728	11.68 — 10.12 = 1.56	1728	1855	11.68 — 10.80 = 0.88	1877	1917	11.80 — 10.52 = 1.28
June	1695	1730	14.83 — 13.45 = 1.38	1730	1814	14.83 — 13.93 = 0.90	1923	1939	14.65 — 13.88 = 0.77
July	1692	1780	16.48 — 15.31 = 1.17	1780	1842	16.48 — 15.31 = 1.17	1888	1949	16.30 — 15.47 = 0.83
August	1692	1800	16.13 — 14.78 = 1.35	1800	1814	16.13 — 15.02 = 1.11	1846	1938	16.40 — 15.09 = 1.31
September	1692	1730	14.22 — 12.02 = 2.20	1730	1838	14.22 — 12.78 = 1.44	1912	1944	13.82 — 12.89 = 0.93
October	1694	1730	10.04 — 8.64 = 1.40	1730	1843	10.04 — 9.03 = 1.01	1889	1964	10.76 — 8.74 = 2.02
November	1680	1729	6.76 — 5.25 = 1.51	1729	1785	6.76 — 5.26 = 1.50	1860	1946	6.84 — 5.26 = 1.58
December	1676	1735	4.40 — 2.60 = 1.80	1735	1799	4.40 — 3.13 = 1.27	1885	1915	4.85 — 3.33 = 1.52
$\bar{Y}$	1690	1741	$\bar{\Delta t} = 1.58$	1741	1804	$\bar{\Delta t} = 1.22$	1886	1936	$\bar{\Delta t} = 1.28$
$s$	$\pm 7$	$\pm 23$		$\pm 23$	$\pm 37$		$\pm 20$	$\pm 15$	
$\bar{M}$	1694	1731		1731	1786 1844		1888	1934	
$\frac{1}{2}W_c$	$\pm 3$	$\pm 10$		$\pm 10$	$(\pm 14)^*$		$\pm 10$	$\pm 20^{**}$	

Note.  $\Delta t$  is the temperature difference between the relative minimum and relative maximum,  $\bar{Y}$  the mean year of occurrence of the relative monthly extremes,  $s$  the standard deviation of the initial years,  $\bar{M}$  the mean initial year, referred to the modal class, of the relative monthly extremes,  $\frac{1}{2}W_c$  and  $1\frac{1}{2}W_c$  the measure of variation, referred to  $\bar{M}$ , and  $\bar{\Delta t}$  the mean of  $\Delta t$ , referred to the monthly values.

\* For a bimodal distribution  $\frac{1}{2}W_c$  is unrealistic, and for a uniform distribution it even became necessary to adopt the values  $2W_c = \pm 56$ .

\*\*  $1\frac{1}{2}W_c$  here, because of uniform distribution across 3 classes.

occurrence and a standard deviation of  $\pm 7$  a for this year. However, when Sturges's formula (Sachs 1974) is used to divide the years into four classes each having a duration of six years each, namely 1674–79, 1680–85, 1686–91 and 1692–97, the resultant class frequencies are 1, 2, 0, 9.

This distribution differs significantly from a Gaussian normal distribution (it is the so-called  $J$ -distribution), therefore the given arithmetic mean of all the years must be replaced by the arithmetic mean of the years belonging to the modal class (the class of highest frequency). This mean value is 1694, in agreement with the corresponding year of the annual values. It appears that instead of the standard deviation of all the years of occurrence of the monthly values it is the half-class-width,  $\frac{1}{2}W_c = \pm 3$ , which should be used as the appropriate measure of variation. If this is interpreted as the range of the values, then  $1694 \pm 3$  embraces the entire modal class.

With reference to the low-pass filtered data, the relative maximum of the annual values observed in 1732 occurs less consistently in the monthly values. On the other hand, however, the frequency distribution of the initial years does differ significantly from a normal Gaussian one, again having class frequencies of 10, 0, 1, 1 and a class width of  $W_c = 19$  a. The arithmetic mean for the modal class is the year 1731 ( $\frac{1}{2}W_c \approx \pm 10$ ), only one year before the corresponding year of the annual values.

In relation to the monthly values there is a very inconsistent picture for the relative minimum of the annual values which appeared in 1813, putting an end to the fall in temperature that had taken place after 1732: the mean value occurs in 1804, with a standard deviation of  $\pm 37$  a. The class frequencies are 2, 4, 2, 4 for a class width of 28, and so it is impossible to decide whether it is a case of a bimodal or a uniform distribution. Were it a bimodal distribution the initial years in the two modal classes would be 1786 and 1844 ( $\frac{1}{2}W_c = \pm 14$ ).

The rise in temperature indicated by the low-pass filtered annual values for 1887 to 1946 once again occurs rather more consistently in the monthly trends, but it is in no way as consistent as the rise of 1694–1732. For the relative minimum of the annual values, in 1887, the arithmetic mean of the corresponding monthly data is 1886. If the averaging is carried out solely with reference to the modal class (class frequencies of 2, 1, 7, 2 for a class width of 20) the result is 1888. These computed initial years differ from one another only slightly. (There are no statistically significant grounds for rejecting the hypothesis that the frequency distribution is normal.)

The most recent relative maximum, which is observed in 1946 with reference to the annual values, occurs between 1915 (December) and 1964 (October) with reference to the monthly values. The frequency distribution of the classes (class width = 13) is 4, 3, 4, 1, that is to say, the distribution is nearly uniform. Taking the first three classes together the mean initial year is 1934. In relation to the classes the measure of variation must here be taken as  $1\frac{1}{2}W_c = \pm 20$ .

An example of the inconsistency of precisely this most recent rise in temperature may be seen in the fact that the June values have a minimum, which otherwise occurred on average in 1886–88, during 1923, while in the same year the January values have already reached the relative maximum of the first half of the twentieth century.

Rocznik (1972) investigated the seasonal mean temperature at De Bilt, Potsdam, Vienna and Basel, using (non-running) 10-year means, and equally found significant seasonal differences; at these places the winter temperatures reach their maximum in the decade from 1911 to 1920, and this is in good agreement with the mean of the corresponding years of occurrence in central England for December (1915), January (1923) and February (1917). Rocznik found, however, that the spring, summer and annual values consistently exhibit their relative maximum in the decade from 1941 to 1950, while the autumn values do not peak until 1961–70. This last result coincides with the finding of the present paper for October, namely a relative maximum as late as 1964.

Comparison of the annual trends with those of individual months shows, therefore, that these data show very different variability patterns in the individual climatic epochs. For this reason it is unreliable to use investigations of the rise in temperature during the first half of the 20th century, for example, to draw conclusions having claim to general validity. The most recent relative maximum is itself particularly inconsistent concerning the way in which it occurs in the individual months. As pointed out, a counter example is the relative minimum of  $1694 \pm 3$ .

The results in question will now be related to the zero-dimensional climatic model of Schneider and Mass (1975). This model employs the relative sunspot numbers and the indices of concentration of volcanic dust in the atmosphere (Lamb 1970) as contributory factors. A prominent minimum of the global mean surface temperature occurs in the period from 1670 to 1700, with a much less clearly marked minimum from 1810 to 1830. Both minima may be verified by the analysis here described, as

far as the qualitative relation is considered. For this reason the first-mentioned relative minimum, in particular, but also the subsequent rise in temperature, could be traced back to external influences.

As to the rise in temperature during the first half of the twentieth century, the climatic model of Schneider and Mass does not exhibit a prominent relative maximum, nor does the present analysis of monthly values show a satisfactory consistency in the timing of extremes. It therefore seems that external factors come into the question either on a smaller scale, or even not at all. This holds, equally, for the much-discussed humanly generated influence leading to a rise in temperature through the production of CO<sub>2</sub>, the more so since at present there seems again to be a prevailing drop in temperature. Global considerations, too, show no signs at present of a steady turn-around in the cooling trend (Angell and Korshover 1977; Kukla *et al.* 1977; Schönwiese 1978d).

### Comparison of the variance spectra

The temperature series for central England will now be used to examine the spectral distribution of the variance throughout the entire accessible range of periods  $T > 1$  a, making comparison between the annual means and the corresponding monthly means. Figure 2 shows the selected examples of the variance spectra, while Table III gives an exhaustive list of the periods connected with relatively high variance. Note that the values keep within narrow intervals for shorter periodicities, while for increasing periods the scatter becomes larger. Thus, for a period of '2.2 a' the accuracy is  $c. \pm 0.05$  a; for '5.1 a'  $c. \pm 0.2$  a; for '25 a'  $c. \pm 4$  a; and for '100 a' only 60–200 a.

Consequently, in connection with the variance maxima arising between 67 a and 200 a it is necessary to refer to the analysis of the longer palaeoclimatological series. As an example, Dansgaard *et al.* (1970) found variance maxima at  $c. 80$  a and  $c. 180$  a by analysing a temperature series for Greenland based on estimates derived with an oxygen isotope method applied to an ice-core sample (for a review of palaeo- and neo-climatological methods and results see US Committee for the GARP, 1975). In this range of periods the variance spectrum of the relative sunspot numbers also possesses a particularly significant maximum (Schönwiese 1978a).

The remaining monthly maxima are grouped, also in a very varied fashion, around the following period intervals:

- 22–33 a (March to May are very significant, August to February are missing);
- 9–15 a (mostly of low significance or missing);
- 4.5–8 a (accumulation around 5 a, but mostly of low significance or missing);
- 2.9–3.9 a (very varied, 2.9–3.6 a partly very significant, however); and
- 2.1–2.8 a (2.2–2.4 a, in particular, is frequent and very significant).

The quasi-biennial oscillation which has been very frequently observed, but whose causes remain unexplained, (for a summary see, for example, Kriester 1964; Landsberg 1962; Schönwiese 1969, 1974) is particularly well supported in statistical terms by the present investigation. As to the range of periods  $T > 50$  a, it is necessary to await further palaeoclimatological analyses, which will allow a better determination of the variance maxima arising.

The component of fluctuation which seems to correspond to the double sunspot cycle (the so-called Hale cycle (Dreier 1977)) is certainly less clearly in evidence than the two variance maxima of the quasi-biennial and of the relatively prolonged ( $T > 50$  a) periods but is to be seen in the spectrum of annual values. The same holds true of the fluctuation component which according to Baur (1949) represents a double wave within the sunspot cycle (although the interpretation is very questionable, cf. for example Schönwiese 1976), and especially of the component with periods of around 3 a, which remains to be considered.

**Table III.** *Relative maxima of the variance spectra of annual and monthly means, based on surface air temperature in central England, 1660–1969.*

Year	January	February	March	April	May	June	July	August	September	October	November	December
100(> 95)	200(> 95)	200(> 80)	200(> 80)	67(> 80)		67(> 90)		100(> 80)	100(> 95)	100(> 90)	67(> 95)	100(> 90)
25(> 80)			22(> 90)	25(> 90)	22(> 90)	25(> 80)	25(> 95)	33(> 80)				15(> 80)
[14(> 80)]		12(> 80)	13(> 80)	10(> 80)	10(> 80)	10(> 80)	15(> 80)	14(> 80)	10(> 90)			12(> 80)
	7.7(> 80)	9.1(> 80)	10(> 80)	7.7(> 80)						7.4(> 80)		
[7.4(> 80)]		6.9(> 80)	6.1(> 80)		6.9(> 80)							
5.1(> 80)		5.1(> 80)			5.3(> 80)	5.0(> 80)		5.4(> 80)	5.3(> 80)	5.7(> 80)		5.1(> 80)
									5.0(> 80)	4.5(> 80)		
3.4(> 80)	3.5(> 80)				3.7(> 90)	3.8(> 90)			3.7(> 80)	3.9(> 80)	3.6(> 95)	3.8(> 80)
						3.3(> 80)	3.4(> 80)		3.4(> 80)			3.4(> 80)
						3.2(> 80)						
3.1(> 90)	3.1(> 95)	2.9(> 90)	3.0(> 90)					2.9(> 90)		3.0(> 90)		2.8(> 80)
	2.9(> 90)	2.9(> 90)										
	2.6(> 80)	2.3(> 80)	2.6(> 80)									
	2.3(> 80)	2.3(> 90)										
2.2(> 95)	2.2(> 95)	2.2(> 90)		2.4(> 80)	2.2(> 80)	2.5(> 80)	2.5(> 80)	2.4(> 90)	2.5(> 80)	2.5(> 80)	2.2(> 90)	
	2.1(> 80)	2.1(> 90)	2.1(> 80)	2.2(> 95)	2.2(> 80)	2.2(> 90)	2.2(> 95)		2.1(> 80)	2.3(> 90)		
<i>W</i> * 24	252	221	134	93	85	76	78	71	79	103	113	181
<i>A</i> 0.27	0.03	−0.02	0.06	0.17	0.16	−0.03	−0.02	0.12	0.15	0.10	0.10	0.05

\* in  $10^8 \text{ K}^2/\Delta f$

*Note.* Unbracketed numbers give the mean period of the maxima in years, bracketed numbers their significance in per cent, as determined by the  $\chi^2$  test. In addition, the table also contains the variance of the 'white' spectrum *W* and the first-lag autocorrelation coefficient *A*.

In connection with the question of what causes the given prevailing components of variance no point of view will be expressed here beyond Lamb 1972, 1977; Mitchell 1976; Schönwiese 1969, 1974. However, it must be mentioned that the given periods of the prevailing variance components continue to come to light in many investigations, cf. for example Baur, 1949; Dehsara and Cehak 1970; Fleer 1975; Kortüm 1974; Kriester 1964; Landsberg 1962; Landsberg and Kaylor 1976; Markham 1974; Polli 1946; Schönwiese 1969, 1974, 1976, 1978a, 1978c. A synopsis of the statistical structures outlined here seems to bring up the question of external influences, or at least of non-stochastic processes, with reference above all to the range of periods  $T > 50$  a, but also to the quasi-biennial cycle. Otherwise, the results seem to point to internal processes in the climatic system and in the general circulation of the atmosphere, though it is true that the details of the connection remain unclear.

It also remains unclear why the autocorrelation coefficients between months following one another at a distance of one year point partly to 'white' spectra without persistence and partly to 'red' spectra with persistence (Mitchell 1966, Panofsky and Brier 1958). The highest autocorrelation coefficient occurs in connection with the annual values.

The variance integrated over the entire interval of periods  $T > 1$  a, which can be obtained by computing the appropriate 'white' spectra, has highest values in the winter half-year (maximum in January), and lowest values in the summer half-year (minimum in August). Again, this may be the result of feedback processes, in conjunction with the snow cover.

Taken overall, the variance of the monthly mean spectra is much greater than that of the annual spectrum. This means that many of the fluctuations must be of a 'compensating' nature, that is to say, they cancel each other out in relation to the annual variability. In conjunction with Table I this is a further indication that the connection between the monthly and annual values is not particularly good. As already mentioned, the relationships are more favourable in the range of periods  $T > 30$  a or  $T > 50$  a.

A great deal of statistical work is still required, however, to build up a picture, station by station and in neo- and palaeo-climatological outline, of the fundamental characteristics of fluctuation of all the important quantities of the climatic system. The present paper can do no more than make a very small contribution in this direction.

### Acknowledgement

This research was supported by the Deutsche Forschungsgemeinschaft. Calculations were run on the IBM 360/91 computer at the Institut für Plasmaphysik in Garching, near Munich. I am indebted to Herr E. O. Bühler for compiling Figure 2. Figure 1 is composed of plots from the computer.

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## Offshore wind energy systems\*

By P. J. Musgrove

(Department of Engineering, Reading University)

### Summary

It is shown that arrays of windmills deployed in the shallow waters of the southern North Sea could produce 20 per cent of the electricity needs of the United Kingdom. Costings based on 1976 prices show that wind-generated electricity is already competitive with that from oil- and coal-fired power stations.

### Introduction

Wind energy systems deployed in the shallow but windy waters of the southern North Sea have the potential to provide more than 20 per cent of UK electricity needs. With existing experience of windmills, and of aircraft and offshore structures, such wind energy systems could be developed within a relatively short time-scale. A preliminary assessment of the economics of offshore wind energy systems is encouraging, and if action is taken soon we could deploy windmills offshore in large numbers by the late 1980s. Energy from the wind would then be available soon after the worldwide demand for oil is predicted to exceed the maximum available supply, with all the implications that this has for the price of fossil fuels.

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\* Previously published in *Physics Education*, 13, 1978, 210–214.

Before considering offshore wind energy systems in more detail, the limits to the potential of windmills on land are worth review. The power output from a windmill is proportional to the cube of the wind speed, and for low-cost energy production, windmills must consequently be located in areas of high mean wind speed. On land this requires windmills to be sited on hilltops, and in the 1950s the Electrical Research Association surveyed and selected about 1500 suitable hilltop sites, having mean wind speeds at the summit of about  $9 \text{ m s}^{-1}$ . Unfortunately most of these locations (c. 1100) are in Scotland, remote from the main centres of power consumption in the Midlands and the South, and outside the areas served by the Central Electricity Generating Board (CEGB). About 1000 MW of wind generation capacity could, in principle, be installed on the remaining sites in England and Wales, since on average windmills rated at two or three megawatts could be installed on each summit. However, most of these hilltop locations are in National Parks or other areas of outstanding natural beauty, and there would undoubtedly be strong environmental objections to siting large numbers of windmills in such locations. And it must be remembered that the visual impact is not confined to the windmills on each summit, but includes the access road up to the summit and the overhead power lines running from the summit to the nearest suitable grid connection, which may well be 20 km away.

The maximum wind generation capacity, in England and Wales, of about 1000 MW corresponds to about 1.7 per cent of the installed capacity of the CEGB. However, environmental objections could in practice limit the installed wind-generation capacity to well below 1 per cent of the CEGB'S total capacity. It is doubtful whether so small a contribution from windmills on land could justify a national wind-energy program to develop the required megawatt-rated windmills.

### Offshore windmills

The potential contribution from windmills offshore is, however, at least an order of magnitude greater. There is a very large area of windy and relatively shallow water around the coast of the United Kingdom, and the area of the southern North Sea looks particularly attractive, both because it is so shallow and because windmills in this area would be close to the main centres of power demand. Figure 1 shows the very extensive shallow-water area off the Wash. In order to avoid relatively low wind speeds near the coast, and to minimize visual impact, windmills should be deployed at least 10 km offshore. The cost of transmitting power to the shore—about £50/kW for a distance of about 30 km (Denton *et al.* 1975)—provides an incentive not to go too far offshore, but between 10 and 50 km from the shore the area of shallow water off the Wash having a depth less than 20 m exceeds 4000 km<sup>2</sup>.

Figure 2 shows a scaled-up version of the Reading variable-geometry vertical-axis windmill design as it might appear in shallow offshore waters, with the tower extending down to and into the sea bed. Meteorological Office data\* for the offshore area of Figure 1 indicate that the distribution of the duration of wind speed is identical to that shown in Figure 3, and the annual mean speed—at the standard height of 10 m above sea level—is  $7.2 \text{ m s}^{-1}$ . However, the wind speed increases with height above the surface; this variation is usually expressed in the form

$$V_m = cH^\alpha,$$

where  $V_m$  is the mean wind speed,  $c$  is a constant,  $H$  is the height, and the exponent  $\alpha$  is a parameter whose value depends on the roughness of the terrain (see for example Ljungstrom 1976). Over smooth inland areas  $\alpha$  has a value of about 0.30, but for coastal and offshore areas this becomes 0.14. Consequently at the hub height of a multimegawatt offshore windmill, some 50 m above sea level, the annual

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\* Provided by Met 0 3c, Meteorological Office, Bracknell, for light-vessel Dowsing ( $53^\circ 34' \text{ N}$ ,  $00^\circ 50' \text{ E}$ ) and separately for sea area  $52\text{--}54^\circ \text{ N}$ ,  $0\text{--}2^\circ \text{ E}$ .



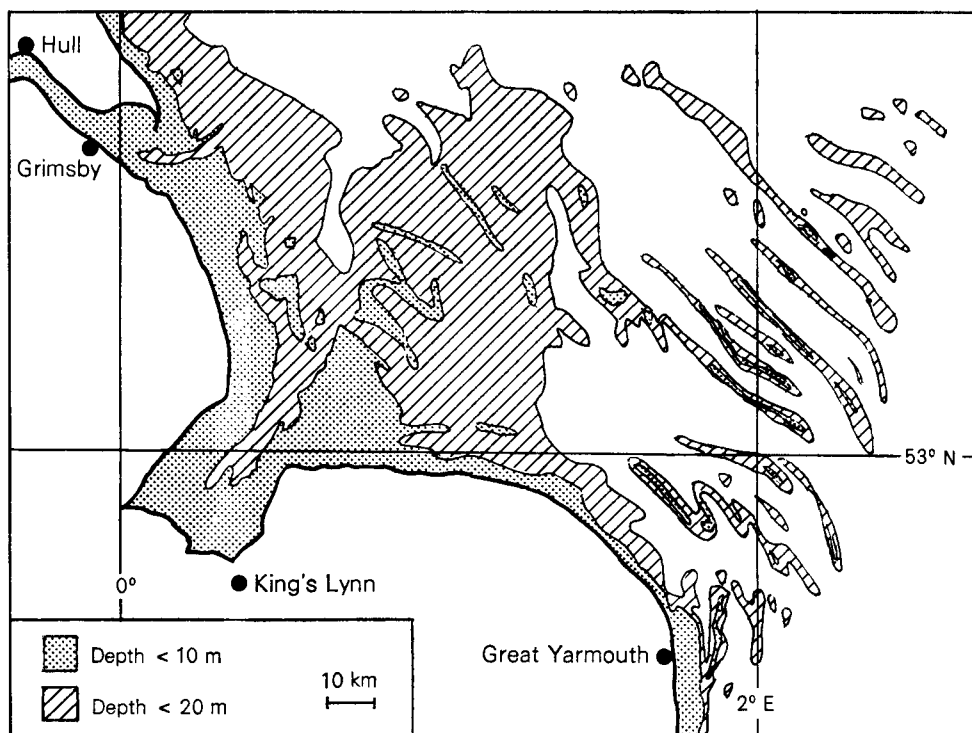


Figure 1. Shallow waters off the Wash.

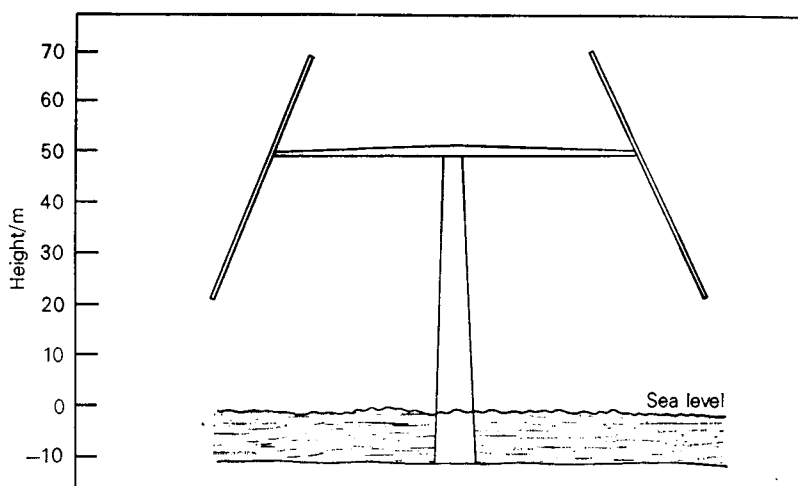


Figure 2. Schematic diagram of a windmill rated at 2.5 MW and having a diameter at hub height of 70 m.

mean wind speed will be  $(50/10)^{0.14} = 1.25$  times the annual mean wind speed at 10 m. For  $V_m = 7.2 \text{ m s}^{-1}$  at 10 m one therefore has  $V_m = 9.0 \text{ m s}^{-1}$  at 50 m, the height of the rotor centre. As stated previously, the power in the wind is proportional to the cube of the wind speed; cubing the ordinates of Figure 3 and then averaging therefore allows one to calculate the average power in the wind. It can be shown (Department of Energy 1977) that the annual average wind power is equal to  $2.4 \times (\frac{1}{2} \rho V_m^3)$ , where  $\rho$  is the air density and  $\frac{1}{2} \rho V_m^3$  is the power in the wind at the annual mean wind speed. A mean wind speed of  $9.0 \text{ m s}^{-1}$  consequently corresponds to an average wind power density of  $1100 \text{ W m}^{-2}$ .

The 70 m diameter windmill shown schematically in Figure 2 has a rated power output of about 2.5 MW in a  $13.5 \text{ m s}^{-1}$  wind ( $1.5 \times$  average wind speed) and will give an annual load factor of about 40 per cent (that is to say, the annual average power output will be 1 MW). One of the attractions of wind energy is the fact that wind speeds are higher in winter than in summer, and the seasonal availability of wind energy closely matches the seasonal variation in electricity demand. The average power output from a windmill (and hence its load factor) is consequently significantly higher in winter than in summer. Of course, a conventional horizontal-axis windmill, such as that shown in Plate I, could be used offshore instead of the more novel vertical-axis windmill shown in Figure 2. For the same rated power output it would have a similar diameter.

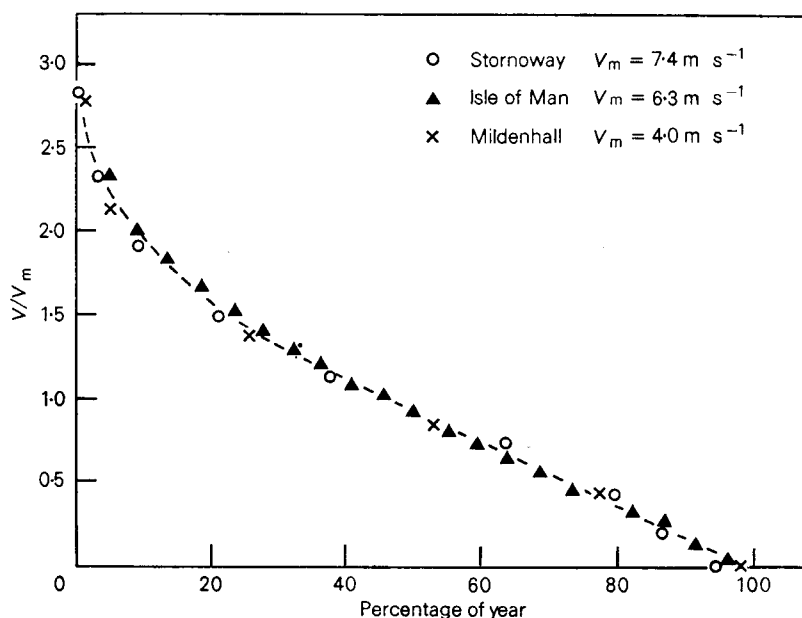


Figure 3. Distribution of duration of wind speed.

### Windmill clusters

To facilitate supervision and maintenance, and to minimize the cost of interconnecting the power outputs from individual windmills, offshore windmills should be deployed in clusters of about 1000 MW total installed capacity. A windmill cluster based on the 2.5 MW design outlined in Figure 2 would therefore consist of about 400 windmills. Golding (1976) suggests that the distance between large



Plate I. Two-hundred foot diameter wind turbine rated at 2 MW. Completed in summer 1979 at Boone, North Carolina. Constructors General Electric Co. Blades by Boeing Engineering and Construction.



windmills should be about eight diameters, and recent Swedish studies (Ljungstrom 1976) suggest a spacing of about seven diameters. The distance between 70 m diameter windmills should therefore be about 0.5 km. A 400 windmill cluster, in its simplest form, would then be a  $20 \times 20$  grid occupying an area of  $10 \text{ km} \times 10 \text{ km}$  (easily visualized on Figure 1). In practice the shape of the cluster would be modified to take advantage of sea-bed contours and the fact that some wind directions occur more frequently than others. If an inter-windmill spacing of ten diameters were considered more appropriate (there is still some uncertainty about the optimum spacing between windmills in a cluster) the 400 windmill cluster would require an area of  $14 \text{ km} \times 14 \text{ km}$ , which is less than five per cent of the total hatched area shown in Figure 1.

One such windmill cluster would have a rated power output of 1000 MW and give an average power output of 400 MW. These figures are comparable with the output of conventional (oil- or coal-fired) power stations on land\*. One windmill cluster would provide 1.7 per cent of our annual electricity needs, equivalent to approximately 1.7 million tons of coal, or 1.0 million tons of oil, per annum. In the area off the Wash one could locate several such windmill clusters, and other locations around our coasts, for example Cardigan Bay, could provide additional shallow-water sites for offshore windmill clusters. The total wind power potential, for clusters sited in water no more than 20 m deep, is approximately 20 per cent of our present electricity requirements, and if it proves economic to deploy windmills in rather deeper water, for example up to 30 m deep, this potential can be doubled.

Wind energy systems therefore have the potential to provide a significant proportion of our electricity requirements, and their environmental impact is minimal. However, it must be recognized that wind energy systems do not provide a firm power output unless they are associated with large-scale energy storage systems, for instance surface or underground hydroelectric energy storage. Although such storage schemes are at present under development in the United States of America, their costs are still uncertain. Initially at least, the development of offshore wind energy systems must be justified by their use without any energy storage.

Windmills without energy storage provide an output on an intermittent basis, which would allow some of the least efficient fossil-fuelled power stations to be shut down on windy days, so saving oil or coal. The amount of intermittent wind generation capacity that can be connected to the grid system is about 12 000 MW. This represents about five thousand 2.5 MW windmills within the United Kingdom, and the export potential, in Europe and elsewhere, is even greater. Even without energy storage 120 000 MW of UK wind generation capacity would provide about 20 per cent of our electricity needs and save approximately 12 million tons of oil annually. Moreover, calculation of the energy recovery period for large windmills indicates that the energy consumed in their manufacture is recovered well within the first year of their operation.

### **Cost of wind energy**

Offshore wind energy systems could therefore play a very useful role in meeting Britain's future energy needs, and could also provide a major new activity for Britain's aerospace and engineering industries. However, offshore wind energy systems will only be introduced if they can provide electricity at a competitive price. Since the wind itself is free, and is an inexhaustible source of energy, the cost of wind-generated electricity is dominated by the amortization of the windmill system's capital cost.

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\* The 59 000 MW net capacity of the CEBG is provided by 161 power stations and the overall load factor for 1975-76 was 40 per cent. The 20 most efficient fossil-fuelled stations have a net capacity of 26 000 MW and their load factor for 1975-76 was 54 per cent (CEBG 1977).

Since no manufacturer has yet developed a multimegawatt windmill for production in quantity, the capital costs are not known with any precision. However, recent detailed cost studies (Coty and Vaughn 1977, Coty and Dubey 1976) by Lockheed in the USA indicate that the construction and installation on land of a 2 MW rated, 80 m diameter, horizontal-axis windmill would cost \$500/kW if produced in quantity (that is to say, several hundred units). The Lockheed figures relate to a two-bladed variable-pitch design and a rated wind speed of  $12 \text{ m s}^{-1}$  at hub height. The studies also indicate that if the annual mean wind speed at hub height is  $9 \text{ m s}^{-1}$  (corresponding to conditions that offshore windmills in the southern North Sea would experience) the design would give a load factor of 47 per cent. Similar detailed studies by General Electric and Kaman Aerospace (*Proc. Second Workshop on Wind Energy Conversion Systems* 1975, *Proc. Vertical Axis Wind Turbine Technology Workshop* 1976) have produced similar conclusions and cost predictions, and General Electric was subsequently (late 1976) awarded a contract to build its 2 MW windmill design, which was completed in 1979.

At the current exchange rate of  $\text{£}1 \approx \$2$ , the windmill cost of \$500/kW corresponds to £250/kW. This includes approximately £50/kW for the tower cost, on land. In an offshore location the tower cost would be substantially increased, and initial estimates suggest that the extra cost of offshore towers would be about £100/kW. Transmission to the shore of the windmill cluster's 1000 MW rated power output would add a further £50/kW and the total cost of an offshore wind energy system is therefore expected to be about £400/kW, at 1976 prices. The cost per windmill of an offshore windmill cluster would therefore be approximately £1 million, with the tower cost contributing £0.4 million per windmill.

If one assumes a 10 per cent charge rate on the required capital investment of £400/kW (Coty and Vaughn (1977) discuss the range of charge rates that could be applicable in differing circumstances) then the cost of wind-generated electricity is  $1.2 \text{ p/(kW h)}$  (or  $0.33 \text{ p/MJ}$ ). This is somewhat higher than the average cost per kilowatt hour of the fuel burnt in oil- and coal-fired power stations, which for 1975–76 was  $1.02 \text{ p/(kW h)}$  ( $0.28 \text{ p/MJ}$ ). However, most wind energy is provided in the winter months, when power stations low in the merit order have to be operated, and the fuel saving value of wind-generated electricity is more nearly  $1.2 \text{ p/(kW h)}$  ( $0.33 \text{ p/MJ}$ ). Based on these figures\* wind-generated electricity is already competitive with the fuel cost of electricity generated in fossil-fuelled power stations. If one were confident that oil prices would in future remain stable, in real-money terms, and that oil would continue to be available, there would be little incentive to develop wind energy systems, or indeed any other alternative energy systems. However, the Organization for Economic Co-operation and Development (OECD) predicts that the demand for oil will exceed the maximum available supply in the mid-1980s, and recent American studies commissioned by President Carter suggest that the mismatch between supply and demand may occur as soon as 1983. Moreover this world-wide oil shortage will not be a temporary phenomenon, but will become progressively more acute. One can therefore be certain that oil prices, and consequently the price of other fossil fuels, will rise very significantly in real-money terms in the 1980s. What is uncertain is the precise magnitude and timing of these increased prices. However, it will take nearly a decade to develop large wind energy systems to the stage where they can be deployed offshore and make a useful contribution to Britain's energy needs. The necessary development program should consequently be initiated as soon as possible.

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\* Alternatively one may argue that since a 1000 MW offshore windmill cluster would provide an electricity output equivalent to 1 million tons of oil per annum the break-even oil price is £41/ton, that is to say that wind-generated electricity will be economic when the price of oil to the power station exceeds £41/ton. The price of crude oil still continues to rise, and at the time of going to the press (January 1980) was from \$24 to \$32 or more per barrel, i.e. at least £75 per ton.

Fortunately wind energy systems can be developed step by step at relatively low cost. Only after a particular windmill design has been tried and proven and shown to produce electricity economically would one incur the cost of deploying large numbers of windmills offshore.

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## Sixty years ago\*

*Weather Insurance.* The weather is a very important factor in all outdoor occupations and amusements, and the losses involved when adverse weather conditions are experienced amount annually to very large sums. One has only to consider a few instances, such as a promising hay harvest spoilt by rain, corn crops partially ruined by hail storms, building contracts held up by frost, cricket and football matches, and race meetings abandoned owing to frost, snow or rain, to see that the question of Weather Insurance is of great importance.

In the past a certain amount of insurance has been effected against such risks, but the premiums have often been prohibitive, owing to the fact that they were based on a small volume of business and that no reliable data of the average weather conditions had been collated.

The Eagle, Star and British Dominions Insurance Company, Limited, has now, however, opened a special Department, named the 'Pluvius' Department for operating in Weather Insurance, and various forms of policies have been prepared, covering as far as possible all insurable weather risks, including the provision of compensation to holiday-makers in the event of excessive rainfall during the insured period. It is anticipated that there will be a large and growing demand for this kind of insurance.

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[The Eagle, Star and British Dominions Insurance Company was the first company to co-operate with the Office on insurance against rainfall and the first to develop a proper actuarial system for assessing the financial risk.]

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\* *Meteorol Mag*, 55, 1920, 50.

## REVIEWS

*The yachtsman's weather guide*, by Ingrid Holford. 215 mm × 135 mm, pp. 88, *illus.* Ward Lock Limited, London, 1979. Price £3.95 hardback, £1.95 paperback.

This book is generally good on synoptic-scale meteorology and there is a wealth of useful practical advice born of experience. Rather more than half is devoted to chapters on synoptic charts, winds associated with low- and high-pressure systems, and how to make the best practical use of all the information which is available from shipping bulletins and personal observation. Some of these sections are excellent with occasional masterpieces of prose expressing an understanding and appreciation of the behaviour of weather and weather systems of which any professional meteorologist would be proud.

Much of the early chapters is devoted to local winds, sea-breezes, convection, and developing some simple physical ideas about heat and water vapour. Here there is a great variation in quality. Some parts, such as those on atmospheric pressure and cumulus clouds are good, and the author has conceived some novel and useful illustrations to explain things like saturation. But some other attempts at loose descriptions of physical concepts are likely only to mislead.

The weakest paragraphs are those dealing with gusts, winds associated with cumulus and cumulonimbus, and sea-breezes. The author does not distinguish between the airflow in raining and non-raining cumulus, and the diagram of winds near a cumulonimbus (page 45) is wrong. The sea-breeze is not 'dependent on unobscured and strong sunshine'. Weak diffuse sunshine through thin stratocumulus can cause enough warming to produce a good sea-breeze so long as the orientation of the gradient wind to the coast is favourable. The space occupied by largely unnecessary diagrams on pages 56 and 59 would have been much better employed in telling the yachtsmen how and when the sea-breeze is likely to develop offshore.

With the 1979 Fastnet race still very much in mind I looked up 'gale' in the index and found the subject well covered. Definitions of winds include the important reference to gusts: 'gale' being Force 8 with gusts to Force 9, and 'storm', Force 10 gusting to 11. Attention is drawn to the rate of fall of pressure heralding strong winds and to the synoptic situations which often produce gales.

One aspect of gustiness which I have not seen handled in any other book is the so-called 'weight of wind'. The author provides a good explanation of this phenomenon which many yachtsmen have regarded as a mystery. But I am not sure that she is right to dismiss completely the influence of air density.

All in all this is a useful book and good value. The author should be proud of her effort, so it is a pity she does not identify herself in the photograph on page 35.

D. M. Houghton

*Buoyancy effects in fluids*, by J. S. Turner. 215 mm × 140 mm, pp. xv + 368, *illus.* Cambridge University Press, London, 1979. Price £7.95 paperback, also available in hard covers.

In this book, which is written with the needs of both undergraduate and postgraduate students in mind, Professor Turner seeks to explain a range of phenomena which arise in fluids in which there exist (small) density variations. While he has confined his attention to flows in which rotation is not important, the author has included many topics which will be of interest to the meteorologist or oceanographer as well as several which have engineering applications.



Following a brief introductory chapter in which the basic ideas are explained, there are chapters describing linear internal waves, finite amplitude phenomena in stratified fluids and instabilities of shear flows in stratified fluids. A separate chapter is devoted to a discussion of turbulent shear flows. Two following chapters consider the problems of buoyant convection from isolated sources and convection from heated surfaces. The final chapters discuss double diffusive phenomena, mixing across density interfaces and various internal mixing processes. The chapters are each largely self-contained but where the subject requires knowledge of material in other chapters, adequate cross-references are given.

The subjects included necessarily require some mathematical analysis but the author discusses the physical principles involved so that the material should be accessible to a student with a limited mathematical background; some familiarity with the dynamics of homogeneous fluids is however required. Throughout the book a good balance has been achieved between the theoretical ideas and the observations in the laboratory, in the atmosphere and in the oceans. The text is complemented by clear diagrams and many excellent photographs although the insertion of the plates in one place makes reference to them a little inconvenient.

A topic which the reviewer found of particular interest was the development of thermal convection from isolated sources. The treatment, which is typical of that in the other chapters, includes a clear discussion of the development of theoretical ideas which in this case consists of the use of similarity solutions and the assumptions which can be made to treat different situations. This is followed by discussion of other flows in which similar ideas can be used and descriptions of laboratory experiments in which the theoretical ideas may be tested. There are some omissions, in this case the effects of conditional instability which may affect the development of buoyant plumes in the atmosphere, but these do not generally detract from the aim of the book which is to explain the basic ideas.

The material included in the book is rather dated, it being a reprint of a volume first published in 1973, for although some errors have been corrected there has been no updating of the text. This may reduce the usefulness of the book to research workers requiring background material to topics related to their speciality although an updated list of references helps to bridge this gap. The basic principles introduced in this book have not, however, changed and for this reason the book will remain an excellent textbook for teaching purposes; the appearance of this paperback edition will make it accessible to many more students.

P. R. Jonas

*Causes of climate*, by J. G. Lockwood. 240 mm × 150 mm, pp. x + 260, *illus.* Edward Arnold, London, 1979. Price £12.95 (boards), £5.95 (paperback).

This book was written for first and second year university students in geography and environmental sciences. It therefore requires no higher mathematical skills of the reader. The author's desire for a thorough, complete treatment has led to some repetitiveness and tautology, for instance part of page 6 is repeated on page 21, and on page 200 we find 'In many ways drought is the hydrological opposite to flooding'.

After an introductory chapter in which the general climatic system and its elements are defined in a highly condensed but correct discourse, the basic facts about radiation are provided, followed by an instructive chapter on interactions between the atmosphere and the underlying surface, including a mention of Budyko-Sellers global climate models. The following chapter on the atmospheric circulation also includes subsections on the distribution of radiation and on the hydrological cycle. Climatic change and glacial periods are introduced by a slightly unclear description of Lorenz's concept of

'almost-intransitivity', but the rest of the chapter summarizes present knowledge (or opinions) well. Some of the statistics (e.g. Tables 5.2–5.5) show unwarranted precision (though they are all quotations from other authors).

The consequences of climatic change are discussed in Chapter 6 in terms of hydrology illustrated by a hydrological model (hence the expression 'climatic model' in the title: there is no reference here to a global dynamical or radiative model). There is a concise final section on man-made influences on local and global climate. The last chapter is wisely cautious about future climate.

A few mis-statements need correcting. There are not always westerly winds in the stratosphere (p. 36). Blocking patterns have a wide spectrum of lifetimes (p. 131). The correlation between ice cover and insolation is not close in Figure 5.11 (p. 165). The CO<sub>2</sub> concentrations at Barrow and in Scandinavia do not appear in Figure 6.19 to exceed those at Mauna Loa (p. 221). Kondratyev and Nikolsky no longer hold to their sunspot versus solar-constant formula (p. 224, p. 26). There are also a few serious misprints; in Figure 4.21 'subpolar flow' should read 'subpolar low' (twice). On pages 101 and 191 west(ern) should read east(ern). On page 126, 600 m should read 600 km. These and some other errors could mislead the uninitiated. However, these apart, the book is well suited to its intended readers.

D. E. Parker

*The climate mandate*, by W. O. Roberts and H. Lansford. 225 mm × 165 mm, pp. viii + 197, illus. W. H. Freeman & Company Ltd, Reading, 1979. Price £6.70 (hard cover), £3.30 (soft cover).

The authors—one a journalist and one a meteorologist—present a convincing case for the importance of climate and of our understanding of climate and its economic impacts in relation to world food supplies.

Although the book is intended for the general reader, it is largely free of the unjustifiable extreme statements which are common in popular writings on climatic change. Past and present climate, and natural and man-made causal mechanisms are described in an appropriately cautious manner, although the reviewer is not as convinced as the authors about some of the solar–terrestrial influences (p. 77). Seasonal and longer-range forecasting techniques are described in clear terms for the layman.

An entire chapter devoted to recent economic history illustrates the complex links between crops, climate, and social and political factors. Following from this, attempts to modify the weather, by cloud seeding, and to modify crops, by genetic breeding, are described in Chapter 7. The results of cloud seeding are correctly shown to be generally ambiguous. Finally an economic attitude is proposed which synthesizes the views of Malthusian pessimists (who fear the results of overpopulation), social idealists (who believe in Utopia via new social and political structures) and technological optimists. The authors admit that their proposals would involve self-sacrifice on the part of some richer nations, and therefore human nature is what will make their suggestions stand or fall.

There are a few slips. On page 28, 10<sup>-1</sup> year is described as one year instead of 1/10 year. There was snow in south Florida in 1975 (page 83). On page 96 the British GATE ships are omitted, and a non-existent one from East Germany included (the authors used an old GATE plan which was superseded).

The book is a useful guide for those without technical, meteorological or economic background.

D. E. Parker

## **Honour**

We note with great pleasure that in the 1980 New Year Honours List a British Empire Medal was awarded to Miss Mary K. Rope of Leiston, Suffolk, who has sent rainfall readings first to the British Rainfall Organization and then to the Meteorological Office since January 1909.

## **Notes and news**

### **Appointment of new Secretary, Meteorological Office**

Mr Frederick Raymond Howell, M.B.E. (Military), F.C.I.S., has been appointed Secretary, Meteorological Office as from 7 January 1980 to succeed Mr A. C. Hughes. Mr Howell trained as an electronic engineer, and from 1940 to 1967 served in the Royal Engineers as a Regular Army Officer, attaining the rank of Colonel; while in the Army he held both regimental and staff appointments. From 1967 to 1970 he was Principal Officer, Plans and Administration, with Surrey County Council, and then joined the Civil Service as a Principal in the Air Force Department. In 1974 he was promoted to Assistant Secretary in the Navy Department as Head of Naval Personnel Division 2. In his younger days he was a keen Rugby footballer, both as player and referee, played cricket, and sailed. All Meteorological Office staff will wish him well in his new appointment.

### **Successful completion of the data collection phase of the Global Weather Experiment**

The operational year of the Global Weather Experiment ended on 30 November 1979. During that year, which comprised the data collection phase of the Experiment, the most comprehensive and modern observing techniques and data collection systems ever used enabled the 150 Members of WMO to keep the whole atmosphere and the ocean surface under constant surveillance. The Global Weather Experiment, also called FGGE\*, is a major component of the Global Atmospheric Research Program (GARP). The aims of the Experiment are to gain a better understanding of atmospheric motions for the development of more realistic models for weather prediction and to assess the ultimate limit of predictability of weather systems. A closely related aim is to design an optimum composite meteorological observing system for the future. It is also hoped, within the limitations of the one-year period of observations, to investigate the mechanisms underlying fluctuations of climate.

The next step in the Experiment is to complete the collection and quality control of the enormous mass of observational data and to organize it in coherent sets in a form directly usable for research. This work is already in progress and in about two years the full data sets will be archived in the world data centres in Moscow and Washington and will be made available to all research institutes which need them. Research work using some of the data, which has already begun during the operational year, is expected to continue up to 1985 at least.

While it is not possible at this stage to draw any scientific conclusions from this Experiment, the first assessment made by the FGGE Inter-Governmental Panel which met in Geneva in November 1979 indicates that the observational phase has been carried out very successfully.

Virtually all 150 members of WMO participated actively in the data collection phase, during which the atmosphere was observed more intensively than ever before. The participating countries took special measures to augment their routine data collection activities and several special observing systems were implemented during two Special Observing Periods (January–February and May–June 1979) to provide atmospheric and oceanographic observations from data-sparse areas.

---

\* FGGE is the acronym for First GARP (Global Atmospheric Research Program) Global Experiment. (See *Meteorol Mag*, 1979, 108, 129–134.)

Examples of new observing techniques which were deployed for the Experiment are third-generation polar-orbiting satellites, ocean data buoys, constant-level balloons, special instrumentation for commercial and research aircraft, and special techniques for atmospheric sounding from ships. The performance of these systems exceeded the expectations of the planners of the Experiment, and it is therefore envisaged that it will be possible to use these systems in the future for an improved global weather observing system.

The first assessment which has just been made indicates that the observational requirements stated for the Experiment were met to a large extent and even exceeded in certain areas. The data sets now being processed and assembled into the final format will be the most extensive meteorological data base ever created. The quality of the data has been found to fulfil the accuracy requirements specified by the scientists for the Experiment.

As regards future activities, the data bank acquired during the preceding 12 months provides the necessary basis for the research and evaluation phase of the Experiment. More than 150 scientific institutions in the world will use these data in more than 500 projects during the period 1980–85. The scientific results of these studies are expected to assure the achievement of scientific objectives set for the Experiment.

(Based on a WMO press release)

### **The observational systems of the Global Weather Experiment (summary of the data collection activities during the operational year December 1978–November 1979)**

#### **1. World Weather Watch (WWW) Global Observing System Surface-based:**

##### *Surface-based:*

The global network of all 150 Members of WMO.

c. 9200 stations making observations at the surface.

c. 850 stations making observations in the upper atmosphere.

c. 7000 merchant ships making observations over the oceans.

In addition: 12 specially installed stations on remote islands making observations in the upper atmosphere.

##### *Space-based*

##### *(a) Five geostationary satellites*

Each geostationary satellite views the globe from a fixed position above the equator, taking cloud pictures in the visible and the infra-red wavebands every 30 minutes. From consecutive pictures the cloud motion can be inferred and this is a measure of the average wind velocity during the 30-minute period. Wind observations are therefore the most valuable products from geostationary satellites. The following table gives the average data volume for the entire system:

Satellite	Position above equator	Average number of wind observations per day
GOES–West (USA)	135°W	1400
GOES–East (USA)	75°W	1400
Meteosat (ESA)	0°	850
GOES–Indian Ocean (ESA/USA)	60°E	1500
Himawari (Japan)	140°E	650
Total		5800

*(b) Five polar-orbiting satellites*

Polar-orbiting satellites circle the earth in approximately 100 minutes, scanning the infra-red radiation of the surface and the atmosphere. From radiation data at various wavelengths, temperature and humidity of the atmospheric layers between surface and the stratosphere, which are important basic-state parameters for the description of the atmosphere as a physical system, can be inferred. The following polar-orbiting satellites provided data:

Satellite	Operator	Launch date	Data
NOAA-5	USA	29 July 1976	1000 vertical profiles per day (until December 1978)
TIROS-N	USA	13 October 1978	7500 vertical profiles per day, sea-surface temperature, data collection and platform location system (ARGOS)
NOAA-6	USA	27 June 1979	As TIROS-N
NIMBUS-7	USA	24 October 1978	Stratospheric profiles
METEOR	USSR	25 January 1979	Cloud images

## 2. Observing systems specially implemented for the Global Weather Experiment

Special observing systems were planned and implemented, in particular to cover the vast data-sparse areas over the oceans and in the tropics. Some of these systems—owing to their high cost—could be operated only during two Special Observing Periods (SOPs):

(a) SOP I from 5 January to 5 March 1979.

(b) SOP II from 1 May to 30 June 1979.

*Tropical wind observing ships*

Altogether, ships from 23 countries participated. They were stationed in tropical oceanic regions around the globe, making observations at the surface and releasing balloons at least once daily for atmospheric soundings up to the stratosphere as summarized below:

	Ships	Ship-days	Soundings	Average per day	Surface observations	Average per day
5 Jan.–5 Mar.	40	1480	3142	52	5900	98
1 May–30 June.	43	1544	3538	58	6200	102

*Aircraft dropsonde system*

Aircraft able to drop sondes measuring wind, temperature and humidity between flight level and the surface operated from various bases over tropical regions and acquired data as summarized in the following table:

Base	Region	Number of soundings	
		15 Jan.–20 Feb.	10 May–8 June
Panama/Acapulco	East Pacific	480	859
Honolulu	Central Pacific	1106	565
Diego Garcia	Indian Ocean	770	677
Ascension Is.	Atlantic	152	492
Total		2508	2593
Soundings per day		68	86

*Tropical constant-level balloons*

Balloons carrying sondes which measure temperature and pressure were released from bases on tropical islands and floated at a constant level of approximately 15 km above sea level. A satellite-borne

data-collection and location system (ARGOS) acquired their data and determined their position from changes in which the wind velocity was computed.

Bases were at Canton Island, Guam, and Ascension Island. A total of 213 balloons was launched in the two Special Observing Periods, providing an average number of 100 wind observations per day.

#### *Southern hemisphere drifting buoy system*

In the vast data-void areas of the southern-hemisphere oceans an improvement was particularly called for. Buoys were developed measuring atmospheric pressure and sea-surface temperature, and they were deployed at predetermined positions. As they drifted through the oceans, their data were collected and their positions were determined by the satellite-borne ARGOS system.

Seven countries contributed buoys to the system.

A total of 368 buoys was launched. At 30 November 1979, 130 buoys were still providing good data.

#### *Aircraft Integrated Data System (AIDS)*

This is a system installed on about 80 wide-bodied commercial aircraft to record automatically wind speed and direction and temperature and pressure along the aircraft's track at intervals of about 200 km.

A total of 650 000 observations was collected. On average this results in 1800 observations per day, distributed almost over the entire globe.

### **3. Summary**

The improvement obtained by the special observing systems in tropical latitudes of the globe shows the following assessment:

Normal *World Weather Watch* (average) resolution accounts for one wind observation every 850 km.

*Tropical wind observing ships* improved the resolution to one wind observation every 700 km.

*Aircraft dropsondes* added a further improvement resulting in one wind observation every 600 km.

*AIDS, satellites, and constant-level balloons* brought the resolution up to one wind observation every 420 km.

*Scientific planners* originally called for one wind observation every 400 km.

The scientific requirements in the tropics, calling for an improvement by a factor of 2, are therefore largely met. In extratropical latitudes, the density of the observations was even better than the requirement.

(Based on a WMO press release).

### **The Ailsa Craig Experiment**

The analysis of the data obtained during the Ailsa Craig Experiment (see *Meteorol Mag*, 108, p. 250) is now complete. Ailsa Craig is a fairly isolated, nearly hemispherical island lying off the coast of Ayrshire. Flow characteristics were recorded with the aid of anemometers on masts, balloon-borne turbulence probes and the C-130 aircraft of the Meteorological Research Flight. During the Experiment mean velocities were measured in a three-dimensional turbulent separated flow—the first time this has been achieved. Several interesting and unexpected features have emerged, notably the presence of upward motion immediately downstream of the island—indicating that flow over the summit does not reattach in this region, contrary to previous ideas. The aircraft measurements have shown that there were powerful trailing vortices downstream of the island, with vertical velocities comparable with

horizontal flow speeds. Such motions have important implications for momentum transfer as well as constituting a hazard to aviation. The results will be assessed in the light of earlier numerical simulations.

### **Recent developments in the implementation of the North West Radar Project**

During December 1979 the first unmanned weather radar in the United Kingdom began to operate at Hameldon Hill near Burnley in Lancashire. This radar, a Plessey 45C (5.6 cm wavelength, 1° beamwidth) has been integrated into a microwave and land-line communications network to form a system which is the basis of the North West Radar Project (a project jointly sponsored by the Meteorological Office, the North West Water Authority (NWWA), the Water Research Centre, the Central Water Planning Unit, and the Ministry of Agriculture, Fisheries and Food).

The aims of the Project include the use of the radar data as an additional source of information for the development of quantitative precipitation forecasting techniques, and an assessment of the benefits of radar data to the NWWA. Data are transmitted in real time to the NWWA, to the Meteorological Office at Manchester Airport, and to the Meteorological Office Radar Research Laboratory at Malvern. At Malvern the data are archived, and some of the data are integrated in near-real-time with the radar data obtained from other radars at Camborne (Cornwall), Upavon (Wiltshire) and Clee Hill (Shropshire). (See *Meteorol Mag*, 108, 1979, 161–184 and 109, 1980, 75–77.)

### **Obituary**

We regret to record the death on 26 December 1979 of Mr D. G. Armour, Higher Scientific Officer. Downie Armour joined the Office as an Assistant in 1939 and for most of the war served first at various RAF stations in the United Kingdom and then in Canada. He returned to the United Kingdom early in 1944 and then, after training as a forecaster, was commissioned and posted to south-east Asia. After demobilization in 1946 he served at outstations until he joined the staff of the Training School in 1954. From 1955 to 1957 he worked at the London Forecast Office in Victory House—the section that was later developed into the London Weather Centre—and became well known to the general public through his appearances as a member of the team of television weather forecasters. After a few years in Germany he returned to his native Scotland, where he worked at the Glasgow Weather Centre for nearly four years and at the Meteorological Office, Edinburgh, for nearly a decade; while he was there he became known to many members of the Scottish agricultural community and built up a large and valuable circle of professional contacts. At the time of his death he was stationed at Edinburgh Airport (Turnhouse).

Downie Armour was a keen Rugby football player in his youth and was a long-standing member of the Boroughmuir F.P. club. He was Secretary and Treasurer of the Scottish Centre of the Royal Meteorological Society from 1967 to 1975, and served as Chairman and Vice-President from 1976 to 1977.

He had a most likeable and friendly personality with a good deal of quiet humour.

### Meteorological Magazine—increase in annual postal subscription

With effect from 4 February 1980 the annual postal subscription rate for the *Meteorological Magazine* was raised from £20.82 to £21.18. This change reflects the recent increase in postal charges; the price for over-the-counter sales remains £1.60 per copy.

### Correction

Examples of banded rainfall distributions in potentially unstable conditions over southern England, by B. A. Hall, *Meteorol Mag*, 109, 1980, 1–17.

In Figure 1(d) as printed on page 5 of the above-mentioned article, an area in the extreme south-west of England is shown with a form of hatching which is not to be found in the associated key.

A corrected version of this Figure is printed below.

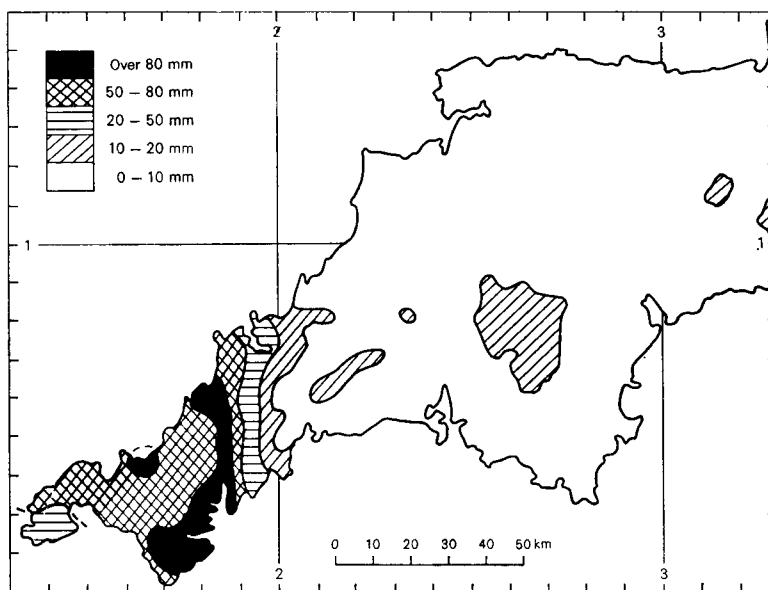


Figure 1(d). Rainfall totals for 24 hours from 09 GMT on 5 October 1977 to 09 GMT on 6 October 1977.





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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

The responsibility for facts and opinions expressed in the signed articles and letters published in this magazine rests with their respective authors.

Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

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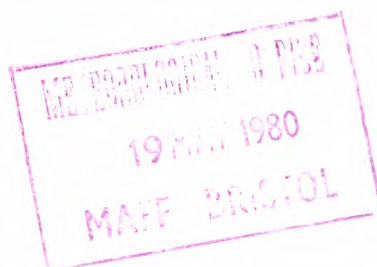
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 3/80

Annual subscription £21.18 including postage  
ISBN 0 11 722060 4  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

May 1980

Met.O. 931 No. 1294 Vol. 109

WPA  
Bf  
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# THE METEOROLOGICAL MAGAZINE

No. 1294, May 1980, Vol. 109

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## **Climatic change or analysts' artifice? — a study of grid-point upper-air data**

By D. E. Parker

(Meteorological Office, Bracknell)

### **Summary**

Comparisons have been made between monthly and annual mean grid-point geopotentials and thicknesses for 20°N to 90°N computed in the Meteorological Office and corresponding values published by the German Federal Republic, USSR and USA. It is demonstrated that the disagreements are large over the oceans and subtropics, and that they are subject to variations and trends which make estimates of hemispheric climatic change unreliable. As a partial remedy it is suggested that a complete and quality-controlled data set of station upper-air observations be created, possibly eliminating some of the heterogeneity over land. However, to resolve the problem over the oceans will require at least the formation of a grid-point data set using an agreed optimum interpolation scheme which must not change with time. Until then, anything smaller than a dramatic hemispheric warming or cooling could escape the notice of users of grid-point data sets.

### **1. Introduction**

The recent upsurge of interest in climatic change and variability has resulted in a pressing need to pay very careful attention to the quality of the data used, and to their interpretation. This paper illustrates the serious difficulties encountered when using grid-point values derived from upper-air data for studies of climatic change in the northern hemisphere.

It has been customary to monitor the earth's climate using data on surface temperature, pressure and rainfall. Surface data have advantages over upper-air data in that they comprise a close network over most land masses, and provide continuity for up to 300 years in a few places. However, in the past 25 years the network density of upper-air stations over land has comfortably exceeded the spatial resolution of mid-latitude synoptic weather systems, and therefore should also be adequate for monitoring climate. Moreover, upper-air data have other advantages: they are relatively unaffected by natural micro-meteorological features such as frost hollows, and by man-made ones such as urban heat-islands.

However, upper-air data suffer from two serious shortcomings. Firstly, the instrumentation, being of recent invention, has been undergoing substantial development, and it is very difficult to distinguish climatic variations from changes of instrumental origin, though given the appropriate information on the instruments it may not be impossible. Secondly—and this applies also to surface data—there are vast gaps in the network over the oceans. Coverage here is limited to a few rawinsonde observations from islands and weather ships, some aircraft reports of variable quality in the upper troposphere, and radiance-based soundings from satellites. Errors in the last appear to be about double the errors in

radiosonde data, using an objective analysis as 'truth' (Atkins 1979). To counteract these problems it would seem wise to make optimum use of the data by determining grid-point values on the basis of subjective or objective analyses which have used all observations simultaneously, from both fixed and mobile stations, preferably weighted according to their estimated quality. These grid-point values will be more reliable if the dynamical relationships between winds and geopotential fields have been used to improve the analysis in areas of sparse or heterogeneous data, and if a reliable forecast field has been used as a 'first-guess' or 'background' field for the analysis. Further improvements can be attained by incorporating corrections for known systematic errors for given stations, and corrections based on time or space consistency.

A serious disadvantage of grid-point values is that, the original data being heterogeneous, it is difficult to assess the quality of the analysis. Also if there is a progressive change in the relative proportions of different types of data which systematically differ, then the grid-point values may show spurious climatic change. This can also occur if the schemes for analysis, or for forecasting from past conditions in data-sparse areas, are changed; or, in the case of subjective analysis, if one analyst is replaced by another.

Angell and Korshover (1978) chose to use upper-air data for fixed observing stations to monitor climatic change of global and hemispheric temperature, and chose stations as evenly spaced as possible. However, their upper-air data set, although probably among the best available for fixed points, still has gaps, mainly over the oceans, sufficient to allow the possibility that variations there could pass unnoticed. Dronia (1974) and Harley (1978) used upper-air grid-point values from different sources and obtained mutually compatible results despite the problems listed above. Their results were also qualitatively compatible with those of Angell and Korshover (1977), quoted in Harley's (1978) Table 2 comparing a variety of authors, even though Angell and Korshover used a deeper layer of the atmosphere. The omission of the Pacific by Dronia may have aided this consistency, though Harley found that omitting the Pacific did not drastically affect the results for the northern hemisphere.

In the following sections the adequacy of upper-air grid-point values for studies of climatic change is investigated by comparing monthly and annual mean geopotential and 'thickness' (i.e. difference of geopotential) fields published by different analysis centres. Only general conclusions can be drawn, because it is not always possible in cases of disagreement to determine which analysis is to be preferred, and because two or more compatible analyses may be based on the same erroneous data. However, the results provide a useful insight into the magnitude of the problems.

## 2. Data

### (a) General description

Mean monthly analyses for 1000 mb–500 mb thickness and for 500 mb geopotential are available both from the (British) Meteorological Office and from the German Federal Republic's Grosswetterlagen (GWL), from the 1940s to date. The analyses published in the USSR *Northern Hemisphere Synoptic Bulletin* include 500 mb geopotential and not 1000 mb–500 mb thickness, so to conduct a mutual comparison, 500 mb geopotential had to be used. The only available source of published USA analyses is the National Meteorological Center 700 mb geopotentials in the *Monthly Weather Review*. These were compared with 700 mb geopotentials in the USSR *Northern Hemisphere Synoptic Bulletin*, and could thereby be compared indirectly with the other nations' analyses which do not include 700 mb geopotential.

The USA and USSR analyses are in the form of contour charts, as are the GWL ones from 1978 onwards. Grid-point values were extracted manually from these, at 10° longitude intervals at 20°, 30°, 40°, 50°, 60°, 70°, 80°, 90°, 100°, 110°, 120°, 130°, 140°, 150°, 160°, 170°, 180°.



40°, 50° and 60°N, at 20° longitude intervals at 70°N, at 60° longitude intervals at 80°N, and at the North Pole. The UK analyses, and the earlier GWL ones, are already in grid-point form.

Because of the interest in current climatic change, and the pressing need to detect, for example, any warming due to increases in carbon dioxide, the aim was to compare analyses for the most recent 5-year period. However, because analyses for 1978 from the USSR were not yet all available, the period 1973–77 was selected for detailed study of monthly analyses, and of annual analyses derived from them. Some comparisons of UK and GWL 500 mb geopotentials and 1000 mb–500 mb thicknesses for 1978 and 1979 are included in order to illustrate the current situation. Also, to provide a longer historical perspective, annual mean 500 mb geopotentials at grid points for 1964–72 available in the GWL were compared with corresponding UK values.

Climatic change is more often monitored in terms of temperature or thickness than absolute geopotential. It will be assumed in this work that differences in 500 mb geopotential analysis can be regarded as approximately equal differences in 1000 mb–500 mb thickness analysis, i.e. that the surface pressure or 1000 mb geopotential analyses agree, which is plausible because the surface data are more plentiful. Trends in differences between 500 mb geopotential analysis can then be regarded as trends in differences between thickness analyses, i.e. as spurious relative warmings or coolings of the atmosphere. The reliability of this assumption has been tested by comparing a chart of thickness differences (Figure 1(e)) with a chart of 500 mb geopotential differences (Figure 1(c)). Clearly the assumption is not unreasonable but neither is it always precise. However, because 1000 mb–500 mb thickness data are not published by two of the four centres whose analyses have been studied, the use of 500 mb geopotential has been regarded as a justifiable substitute.

#### *(b) Details of sources of grid-point values*

(i) *United Kingdom.* The UK grid-point values are based on daily 00 GMT analyses. Since 21 August 1975 a forecast made from the analysis 12 hours earlier has been used as a starting point or ‘background field’ for each 00 GMT analysis and will therefore have had considerable influence in data-sparse regions. In addition, operational subjective intervention has been used to ensure consistency with other levels of the atmosphere (particularly 300 mb where aircraft reports are available), to amend the background forecast when it was mistrusted, to incorporate information not fed directly into the analysis scheme (e.g. from satellite cloud photographs), and to reject or amend data regarded as erroneous.

These processes have been developed gradually over recent years, and this development will be a source of inhomogeneity in the data set. Moreover, although since 21 August 1975 grid-point values have been extracted automatically and objectively from the daily analyses of the 10-level model, previously they were extracted manually from forecasters’ daily hand-drawn charts. A 23-day comparison of the two methods by Folland (1975) showed no serious differences, but areas singled out for possible errors or inconsistencies were the North Pole, the east Pacific, North Africa and Arabia, and the Asian highlands—all of which feature later in this paper.

The radiation scheme of the model forecast was changed at the beginning of 1978.

The UK values for 1964 are in fact derived from the GWL and comparison revealed that the corresponding UK analysis is identical with that published by the Deutscher Wetterdienst.

(ii) *German Federal Republic.* The GWL values are based on daily 00 GMT analyses. The analysis scheme has been automated since the beginning of 1969. No details are published of human intervention, or of the use of forecast fields for guidance in analysis.

(iii) *USSR.* According to the preface to the 1976 and 1977 *Synoptic Bulletins*, ‘the maps were constructed using CLIMAT TEMP data’ (which are monthly mean ascents transmitted from stations).

'Over poorly covered areas of the northern hemisphere these maps were supplemented with data meaned over the month at the nodes of a regular standard geographical grid (with a north-south step of 5° and an east-west step of 10°), which were obtained from daily surface charts and maps of the upper-air pressure topography for 00.00 hours GMT. As the maps were analysed these data were subjected to a correction determined taking account of the magnitude of the difference between the values of the meteorological element at the nodes and at a nearby station (no more than 500 km away), using CLIMAT TEMP data.' The last sentence is not included in the prefaces to the 1973, 1974 and 1975 bulletins. No details are given of human intervention, or of use of forecast fields to assist the daily analyses.

(iv) *USA*. According to Colucci and Bosart (1979) the 6-layer primitive equation model at the National Meteorological Center was replaced in January 1978 by a 7-layer model of higher horizontal resolution. The former model appears to have been operational throughout the period (1973–77) studied here, but clearly the results obtained may not apply to more recent months when the new model was used. Again no details are provided of human intervention procedures, or of the use of forecast fields in analysis. In addition, no indication is given on the monthly analyses whether the data times are 00 or 12 GMT or both.

### (c) *Missing values*

The UK and Grosswetterlagen analyses for 1964 are largely missing over the Pacific, at 20°N over Africa and Asia, and at 30°N over eastern Asia. For 1965 and 1966 the GWL analyses have similar coverage to that of 1964, but the UK analyses, although still omitting much of 20°N, are complete further north except at 30°N over part of the Pacific. From 1967 to 1979 the GWL analyses are virtually complete except for parts of 20°N, mainly over the Pacific. The UK analyses have similar coverage from 1967 to 1972 but are complete from 1973.

The USSR analyses are complete for the period studied, 1973 to 1977.

The USA analyses for 1973 to 1977 do not include part of southern Asia at 20°N, and sometimes it is impossible to extract grid-point values for 30°N over the Asian highlands.

## 3. Results

### (a) *Difference maps*

(i) *UK versus Grosswetterlagen*. Following the separation of the data sources at the end of 1964, the differences in analysis increase not suddenly but gradually until for 1967 the UK-analysed 500 mb geopotentials are greater than the GWL values over most of the area considered, especially North Africa and southern Asia (Figure 1(a)). Similar discrepancies prevail up to 1974 but for 1975 the UK analysis is lower over part of the eastern Pacific. Over the North Pole, the UK analysis begins to be higher than GWL for 1975, and for 1976 this feature is marked, as are extreme differences over North Africa and Arabia (Figure 1(b)); 1977 has an extreme discrepancy of 60 gpm in 500 mb geopotential over the North Pole. For 1000 mb–500 mb thickness this is equivalent to 3 °C. By 1978 the North Pole discrepancy has eased but there are marked differences over the Asian highlands and the eastern Pacific (Figure 1(c)).

The individual monthly 500 mb geopotential difference charts often show discrepancies much larger than the annual charts. Figure 1(d) illustrates a recent 70 gpm disagreement over the Atlantic.

Analyses of 1000 mb–500 mb thickness have been used to produce a chart of relative thickness analysis change since 1973 (Figure 1(f)). It is evident that a British researcher into climatic change would



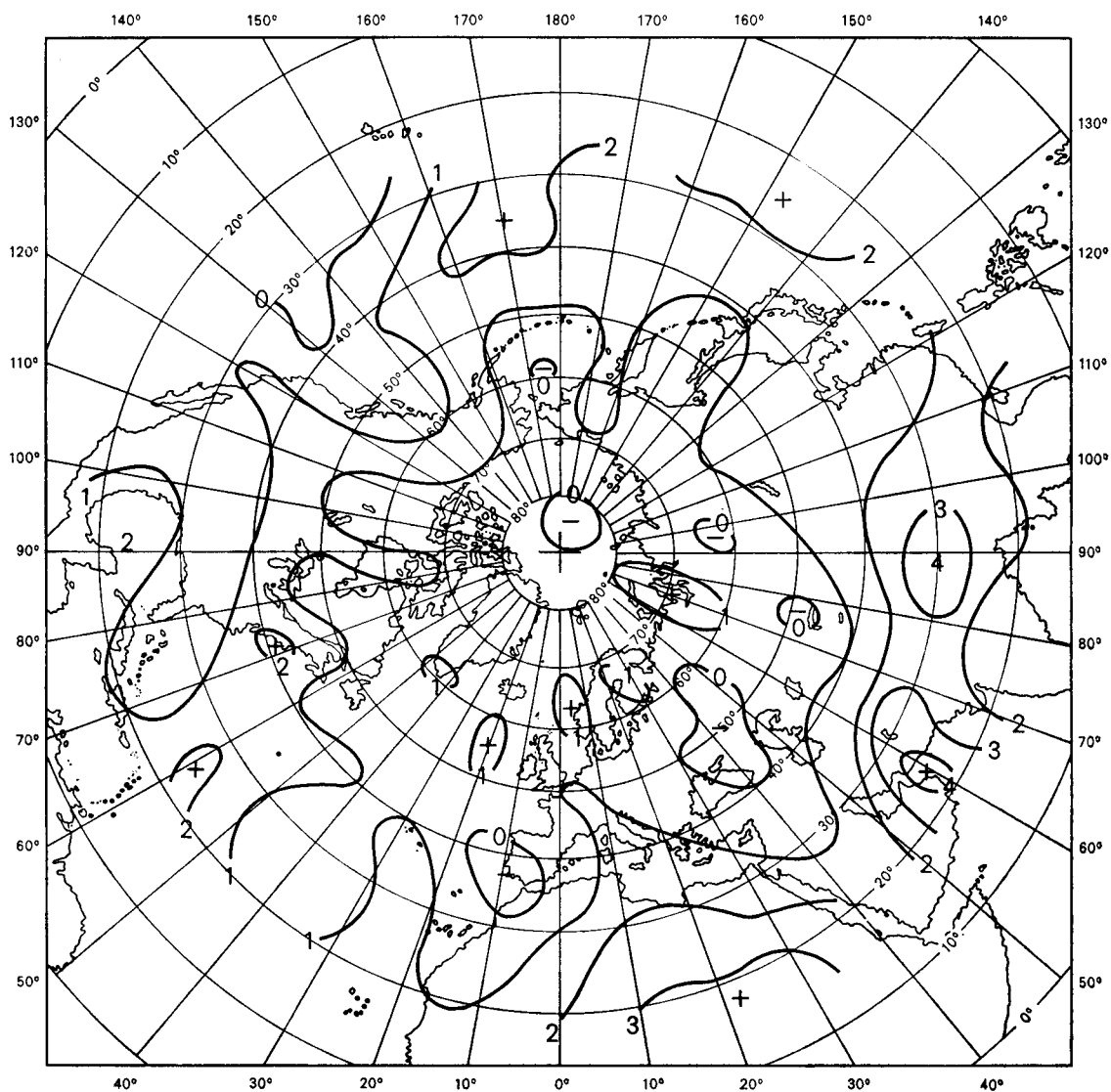


Figure 1(a). Annual mean 500 mb geopotential difference in decageopotential metres. United Kingdom minus Grosswetterlagen (GWL) for 1967, 00 GMT data.

be more likely than a Deutscher Wetterdienst researcher to deduce that the Arctic is warming and the subtropics cooling—and would produce ‘facts’, or so-called ‘data’, in support.

(ii) *UK versus USSR*. Comparison of UK and USSR analyses reveals differences of a similar magnitude to those between UK and GWL, but sometimes of the opposite sign. Figure 2(a), like Figure 1(f), is a chart of spurious climatic change, but for 500 mb geopotential. It has similar features, but they are less marked.

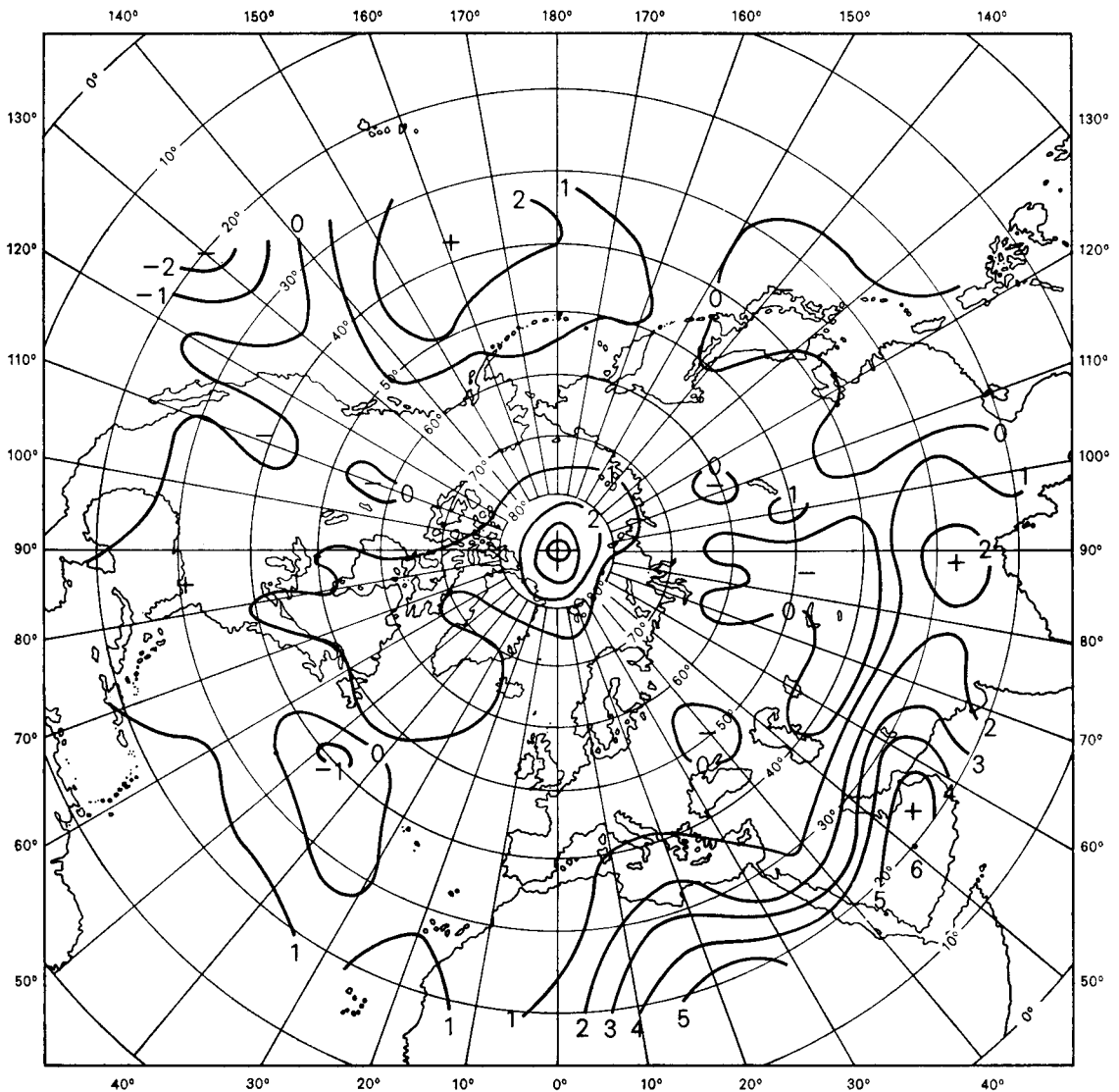


Figure 1(b). Annual mean 500 mb geopotential difference in decageopotential metres. UK minus GWL for 1976, 00 GMT data.

The most extreme monthly difference found is 120 gpm between UK and USSR 500 mb geopotentials over the North Pacific for January 1977 (Figure 2(b)).

(iii) *USA versus USSR*. As stated above, 700 mb geopotential had to be used for these comparisons. The disagreements became smaller in 1977, but even this recent reduction can be interpreted spuriously as climatic change (Figure 3). The effect of a change of layer mean temperature on 1000 mb–500 mb thickness is 1.9 times that for 1000 mb–700 mb thickness, so the contours in Figure 3 are at half the interval of those in Figures 1(f) and 2(a) to give visual comparability in terms of temperature.

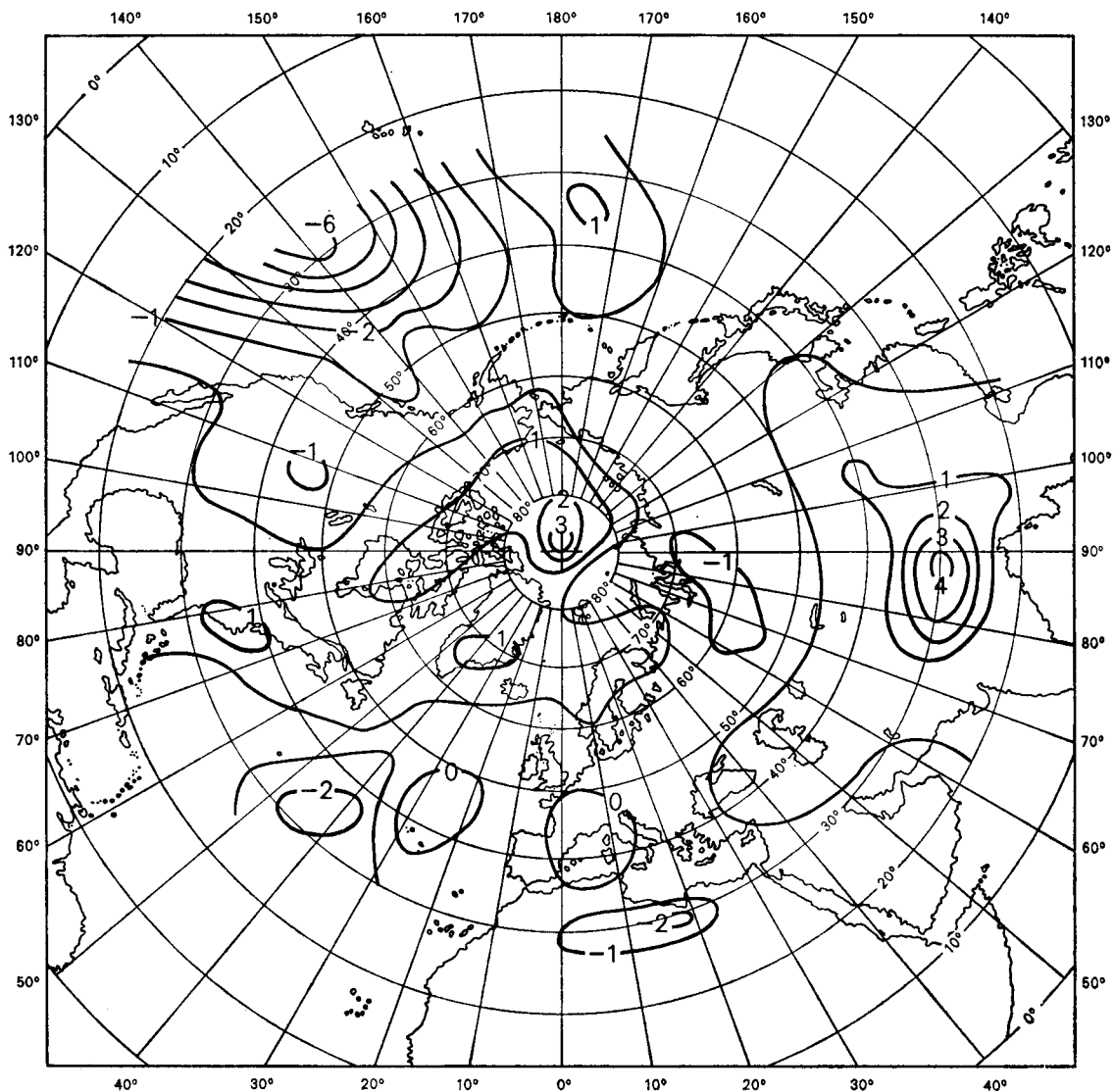


Figure 1(c). Annual mean 500 mb geopotential difference in decageopotential metres. UK minus GWL for 1978, 00 GMT data.

#### (b) Sequences of differences at fixed points

Figure 4 presents monthly sequences for 1973 to 1977 of 500 mb geopotential differences, UK minus GWL, at several fixed points. Because the difference maps are spatially coherent, nearby grid points have similar sequences of differences. Some points (e.g. 60°N 70°W) show a consistent slight difference. Others undergo one or two changes which are sudden (e.g. the North Pole) or gradual (e.g. 30°N 90°E).

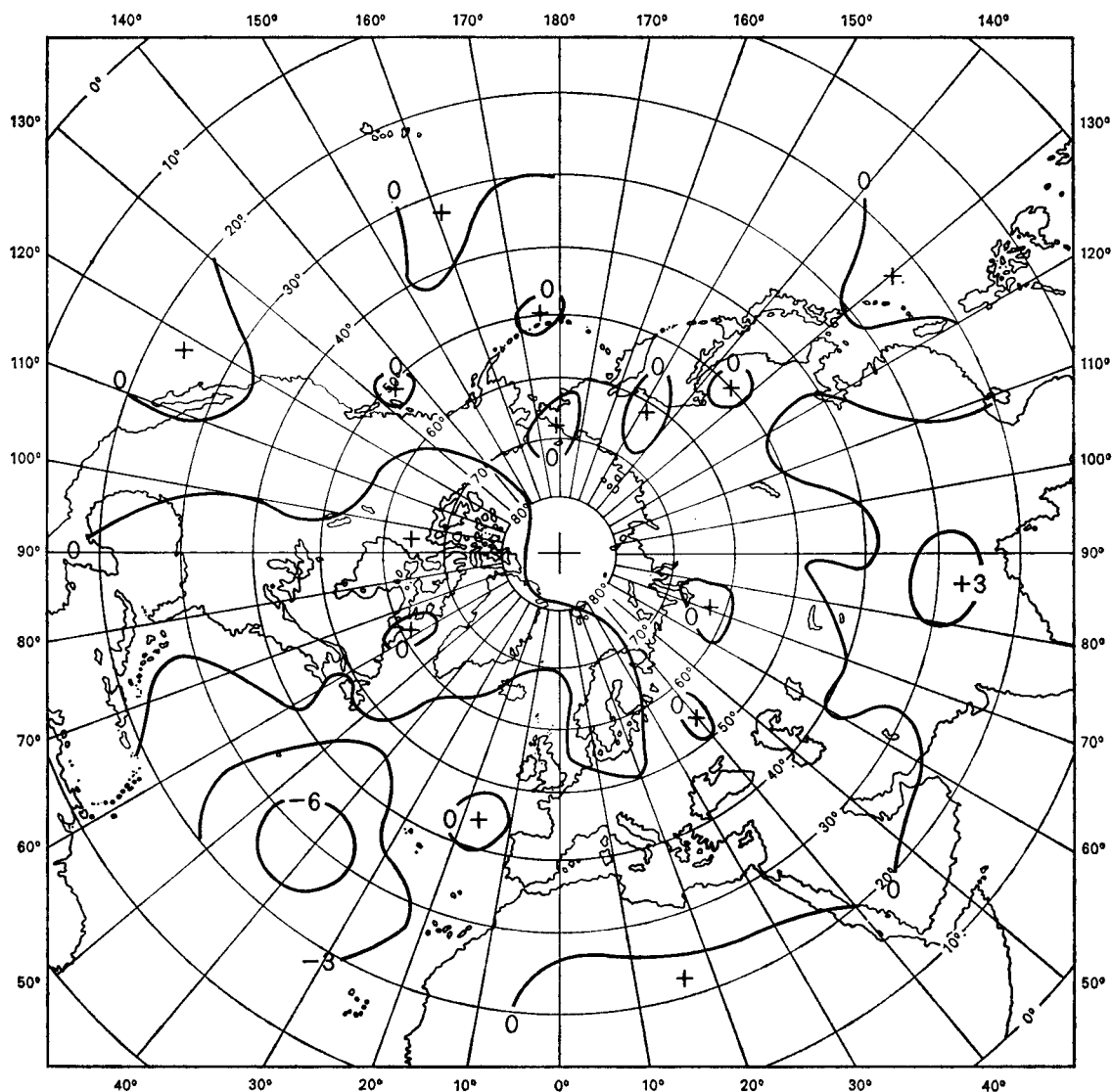


Figure 1(d). 500 mb geopotential difference, UK minus GWL, for July 1979, 00 GMT data. Contours at intervals of 3 decageopotential metres.

At 40°N 140°W there is an apparent annual cycle of differences. At 20°N 50°E the recent marked changes do not conform to an annual cycle.

For the same period for UK minus USSR, the increase at the North Pole is gradual, and there is no systematic trend at 30°N 90°E. The mid-Pacific still has an annual cycle and so do several points at 20°N, mainly for the more recent years. There are several cases of steady systematic differences, e.g. about -25 gpm at 30°N 40°W.

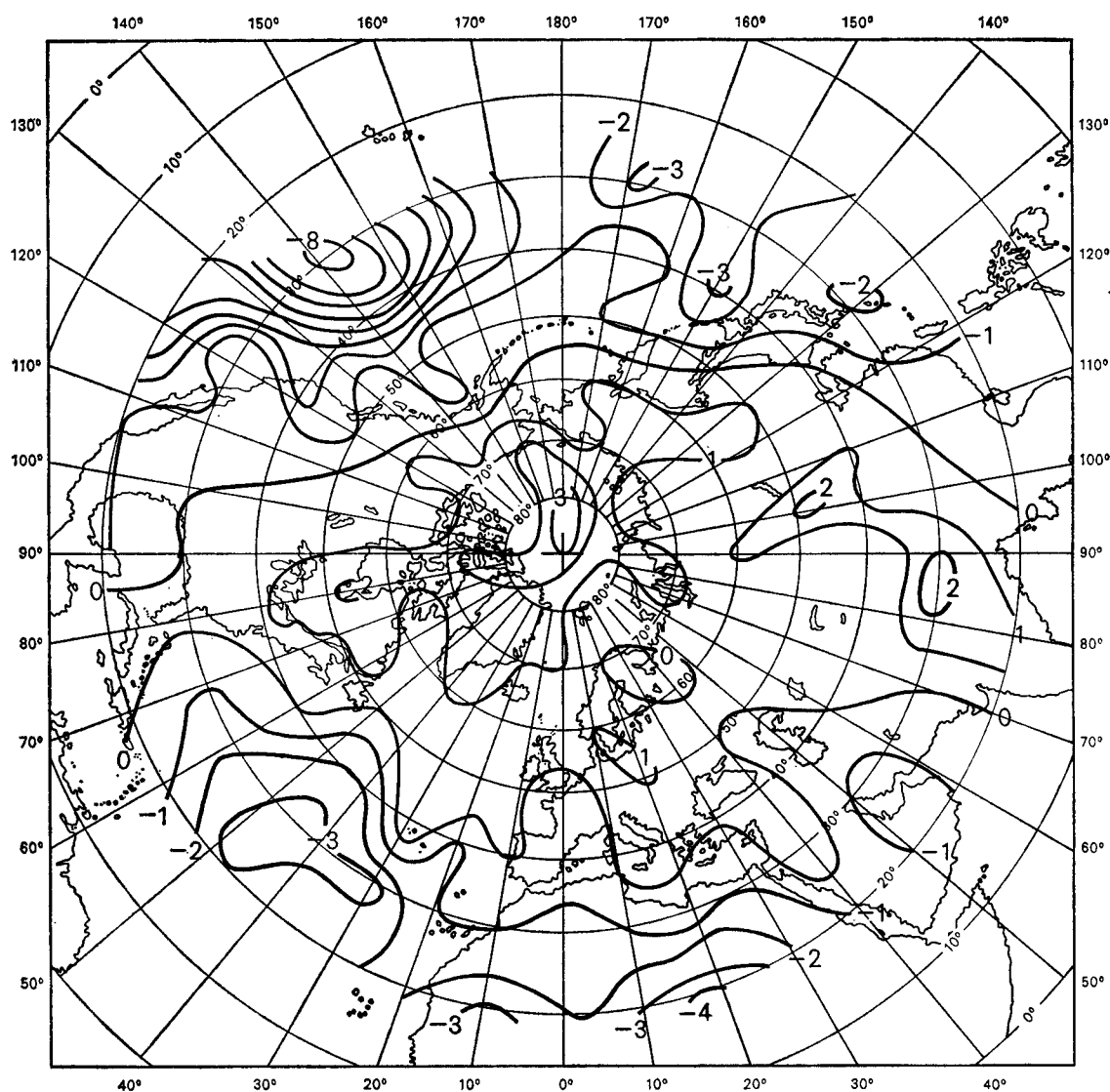


Figure 1(e). Annual mean 1000–500 mb thickness difference in decageopotential metres. UK minus GWL for 1978, 00 GMT data.

USA minus USSR 700 mb geopotential for 1973 to 1977 varies irregularly at the North Pole. There is a systematic decrease of 50 gpm at 20°N 130°W and a consistent –25 gpm difference at 20°N 20°E. There are no clear-cut annual cycles except to some extent at 40°N 160°W.

### (c) Zonal annual mean differences

Climatic change is often monitored in terms of zonal averages. The changes in zonally averaged

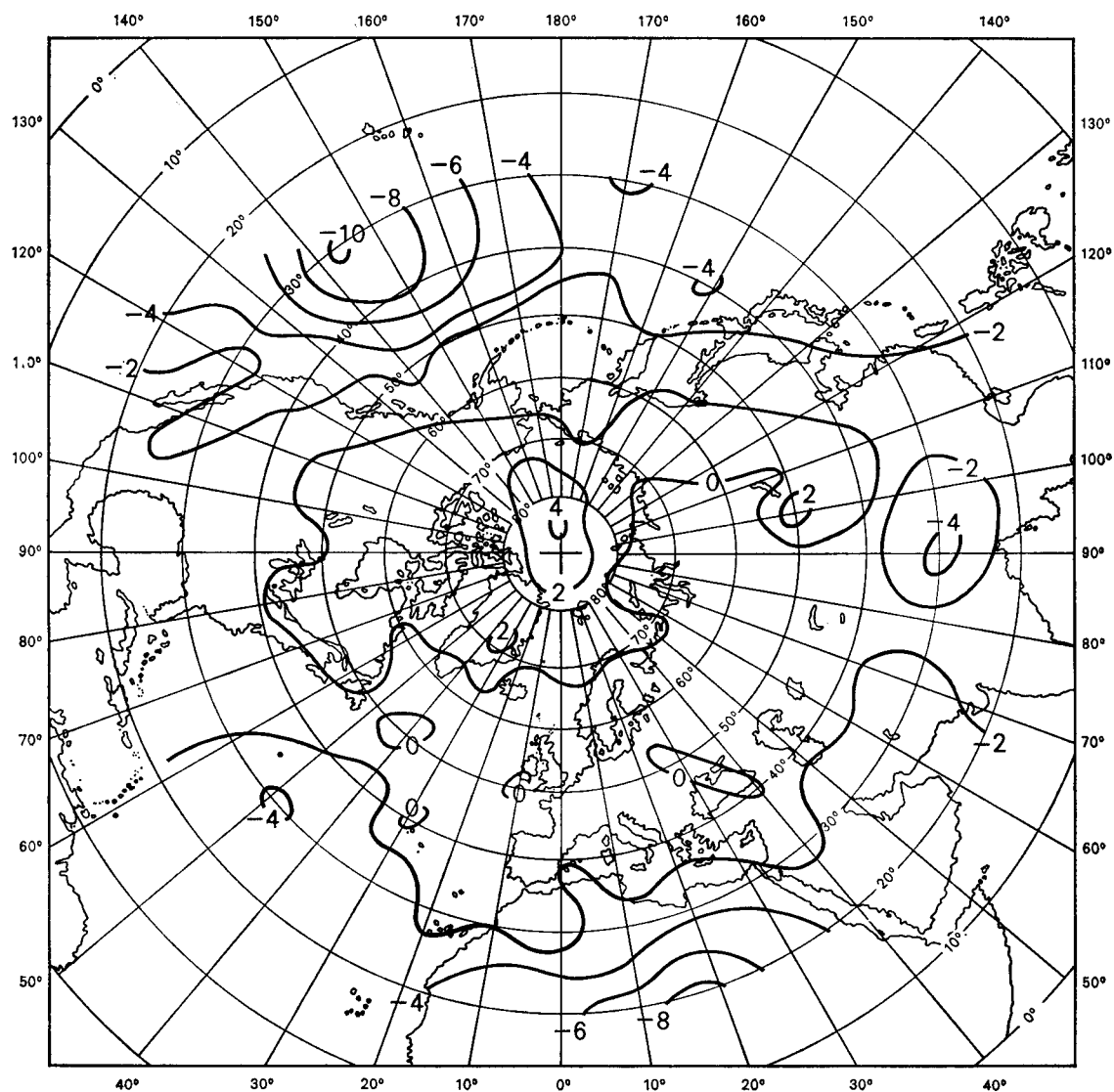
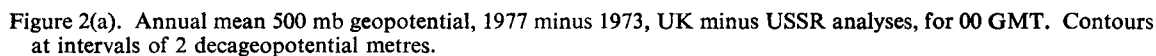


Figure 1(f). Annual mean 1000–500 mb thickness, 1978 minus 1973, UK analyses minus GWL analyses, for 00 GMT. Contours at intervals of 2 decageopotential metres.

annual UK minus GWL 500 mb geopotentials for 1964 to 1978 are shown in Figure 5. Clearly there are large changes at 20°N and 30°N and near the North Pole, but even mid-latitudes are not immune, showing a relative reduction of UK geopotentials in the most recent years. The changes in 40°N minus 60°N 500 mb geopotential differences are unsystematic and correspond to geostrophic wind differences of less than 1 m/s.

Trends in low latitudes and near the North Pole are also characteristic of the 1973 to 1977 zonally averaged annual UK minus USSR 500 mb geopotentials and USA minus USSR 700 mb geopotentials.



(d) *Areal annual mean differences*

Figure 6(a) summarizes the changes in the annual geopotential differences averaged over the whole analysis area. The zonal means are weighted according to the cosine of the latitude (cosine  $80^\circ$  is used for the North Pole). The weights have been multiplied by the proportion of coverage of grid-point values at that latitude. The USA minus USSR 700 mb geopotential differences have been multiplied by 1.9 as a means of converting them to effective 500 mb geopotential differences.

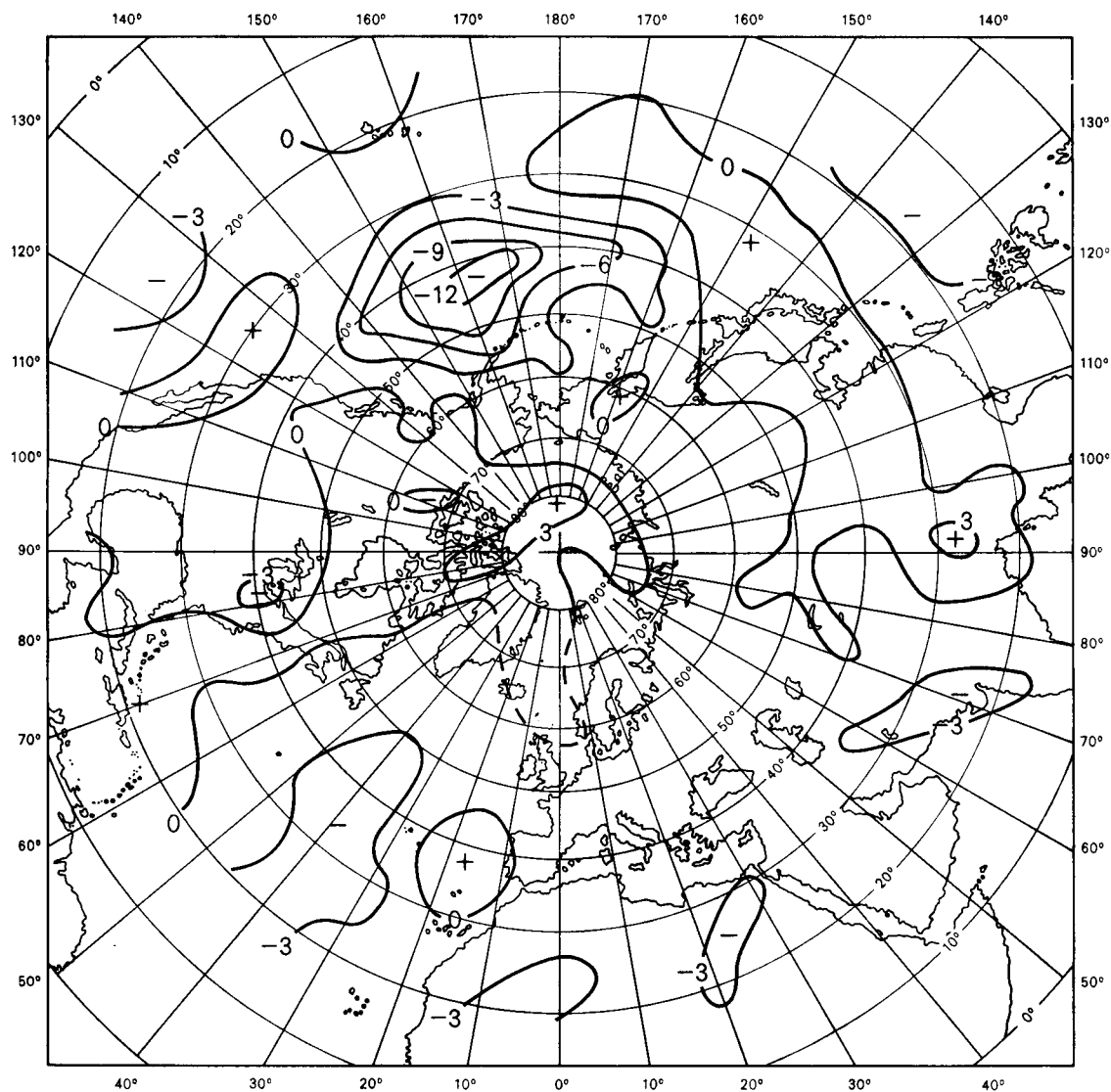


Figure 2(b). 500 mb geopotential difference, UK minus USSR, for January 1977, 00 GMT data. Contours at intervals of 3 decageopotential metres.

The geopotential scale in Figure 6(a) includes an effective temperature scale which applies if the differences are assumed to be the same for 1000 mb to 500 mb thickness. The recent trends of the differences are of the order of 0.5 °C in these terms.

(e) *Annual thickness or temperature equivalents*

Figure 6(b) presents the annual areal mean 1000 mb–500 mb thicknesses for 1973 to 1978 as derived



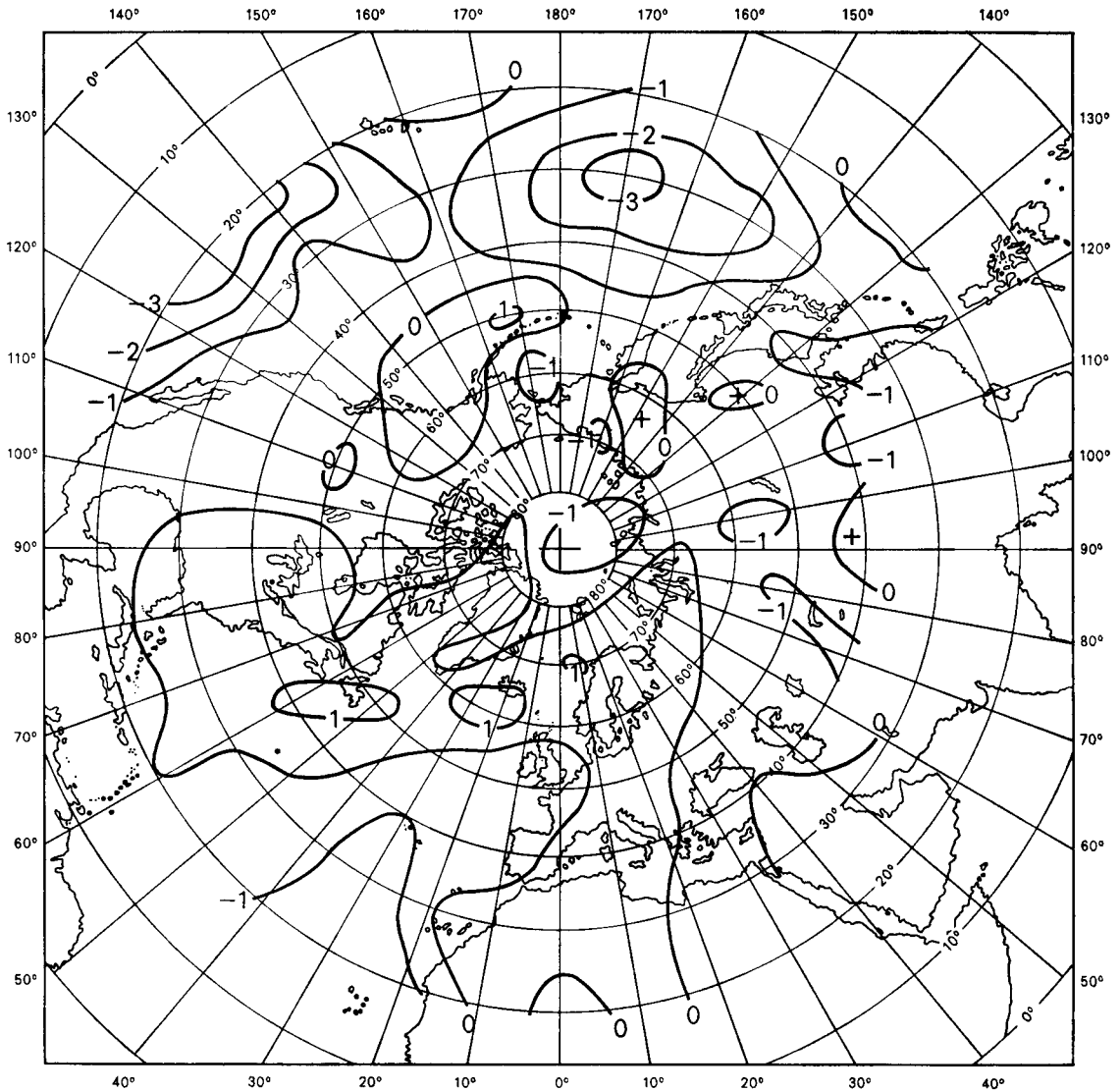


Figure 3. Annual mean 700 mb geopotential, 1977 minus 1973, USA analyses minus USSR 00 GMT analyses, in decageopotential metres.

from the UK analyses for the whole analysis area. The estimated GWL and USSR thicknesses have been derived from these by subtracting the areal annual mean 500 mb geopotential differences. The estimated USA thicknesses have been derived from the estimated USSR thicknesses by adding 1.9 times the areal annual mean USA minus USSR 700 mb geopotentials.

The conclusion is obvious: we can detect interannual warmings and coolings, whether they are real or of instrumental origin or due to geographically biased data. But the changes in the analysis differences

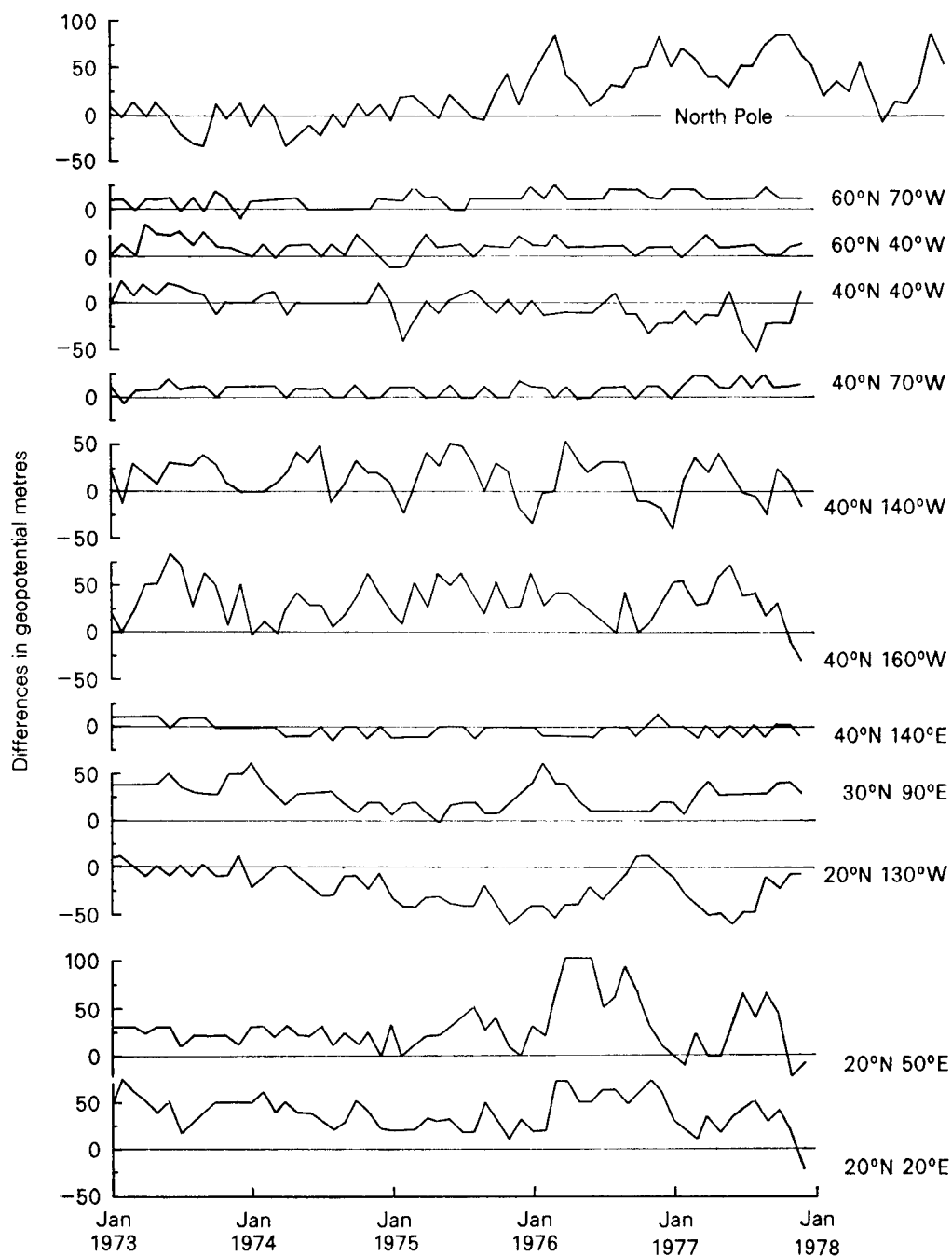


Figure 4. Monthly mean 500 mb geopotential differences, UK minus GWL analyses.

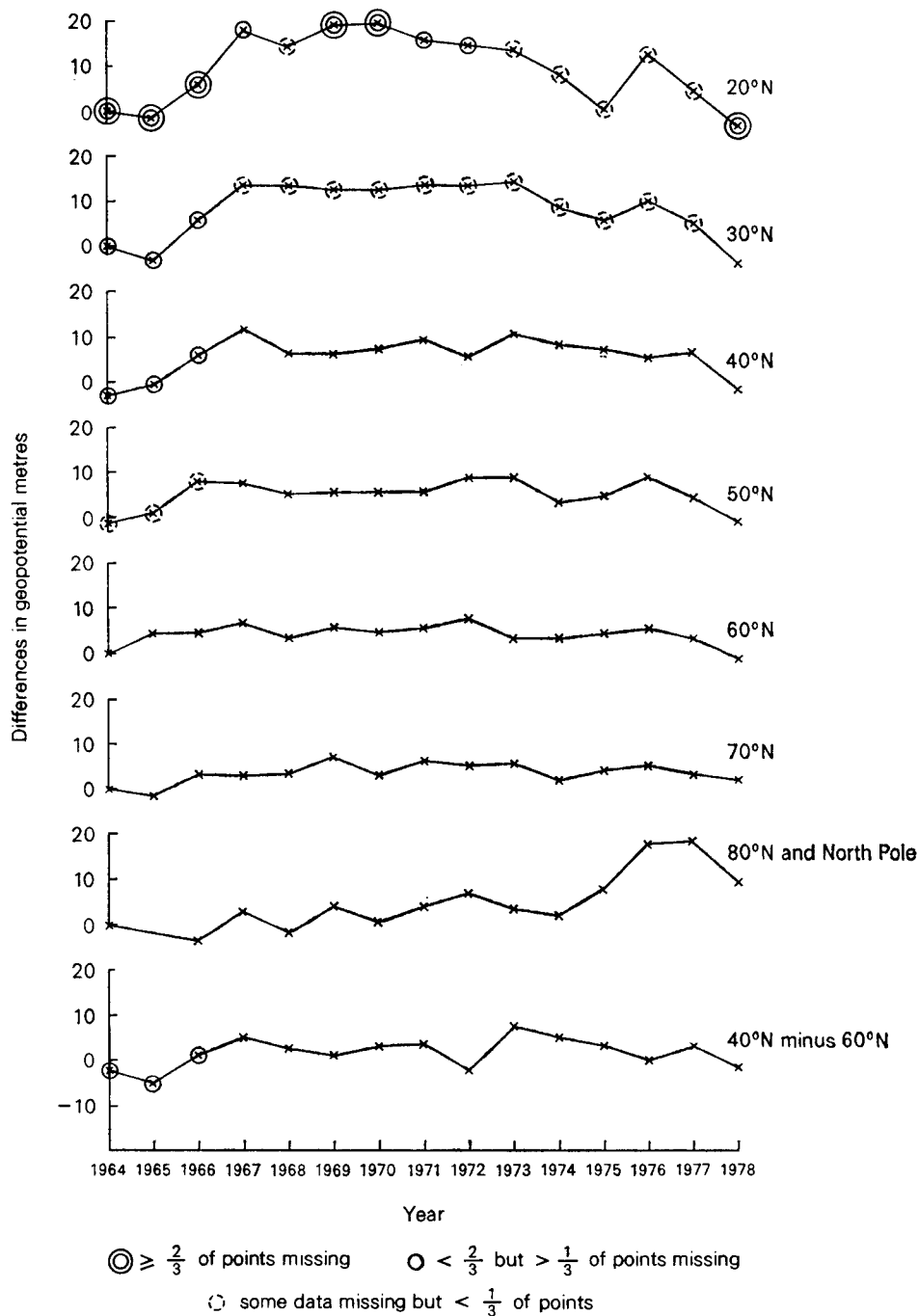


Figure 5. Zonal annual mean 500 mb geopotential differences, UK minus GWL analyses.

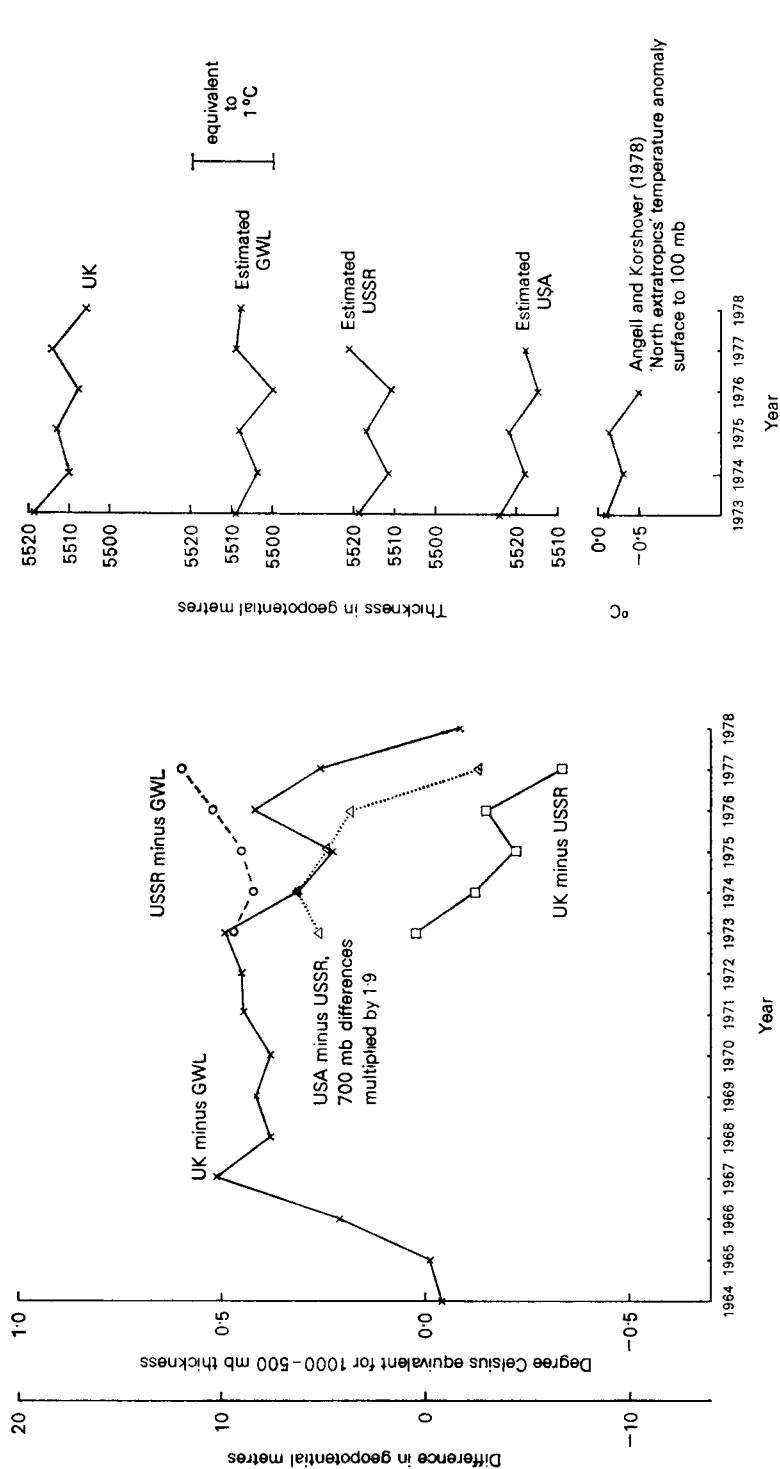


Figure 6(a). Annual mean differences between national 500 mb geopotential analyses, areally meaned from 20°N to the North Pole. Note. Until 1972 inclusive, and in 1978, much of 20°N was missing. Until 1966 inclusive much of 30°N and 40°N were missing.

Figure 6(b). Annual mean thickness of the 1000-500 mb layer, areally meaned from 20°N to the North Pole.

are large enough to swamp any subtle climatic trends for the hemisphere. Has it cooled, as the UK and USA graphs suggest, or has the cooling been arrested as indicated by GWL and the USSR?

Figure 6(c), derived in the same way as Figure 6(b), shows that the relative trends in 1000 mb to 500 mb thickness are largest at 20°N and in the Arctic and least at 50°N and 60°N. Concern about warmings and coolings in the Arctic, if regarded as being precursors of changes elsewhere, needs to be matched by great care in using the data.

*(f) Seasonal trends*

Zonally and areally meaned 500 mb geopotential differences for January and July for UK minus GWL (not shown) display trends which are similar to one another and to the annual trends in Figures 5 and 6(a), though in summer the decrease in the subtropics is later and more sudden. The UK minus USSR differences change much less markedly in summer than in winter. The USA minus USSR differences behave differently in the opposite seasons but the variations in summer are only a little less marked than in winter.

Sequences of January and July zonally and areally meaned 1000 mb–500 mb thicknesses, derived in the same way as in Figures 6(b) and 6(c), but not illustrated here, show best agreement between analyses in mid-latitudes and poorest in the subtropics, with changes of differences between analyses still swamping any real climatic change which may have occurred for the composite area 20°N to the North Pole.

*(g) Comparison of trends and changes of annual mean thicknesses with results of other workers*

Figure 6(b) includes temperature anomalies for 1973 to 1976 for the northern hemisphere extra-tropical latitudes, derived from Angell and Korshover (1978). It should be noted that Angell and Korshover's results are for the surface to 100 mb and are for stations (not grid points) north of 30°N, and mainly north of 40°N. The results are more like the thickness series for GWL and USA than the other two series. The presentation of their results in Figure 6(b) was derived by averaging their four seasonal temperature anomalies which they had already time-smoothed. Because the winter season overlaps the turn of the year, the annual values attributed to Angell and Korshover in Figure 6(b) refer to the 12 months beginning with December of the previous year.

Figure 6(d) presents surface to 100 mb temperature anomalies for 1964 to 1976 for the north subtropics (stations mostly near 20°N), the north temperate zone (stations mostly near 50°N) and the north polar zone (stations mostly near 70°N), derived from Angell and Korshover (1978) in the same manner as for Figure 6(b). The north subtropics series is similar to the 30°N series in Figure 6(c) but with some marked disagreements, e.g. having 1968 colder than 1967. The north temperate time series has smaller recent fluctuations than the 50°N series, but they are in phase. The north polar series parallels the 70°N to North Pole series during 1968–73 but bears little relation to it outside this period, or to the '70°N only' series added to Figure 6(c) in order to allow a more direct comparison.

Figure 6(e) compares annual mean temperatures of the 1000 mb–500 mb layer at high latitudes for 1973 minus 1949 presented by Dronia (1974) with values computed from the UK grid-point data, used by Painting (1977). The greater relative cooling according to Dronia is consistent with Figure 6(c): Dronia used GWL grid-point values.

In conclusion, the general similarity of the results of Angell and Korshover to those obtained from grid-point values shows that uncertainties of analysis have not overridden the information contained in the original data; but the differences between the various estimates make it impossible as yet to resolve hemispherically meaned climatic trends. Moreover, any systematic instrumental changes in the instrumental data may have contaminated all the estimates to a similar but unknown (though not unknowable) degree.

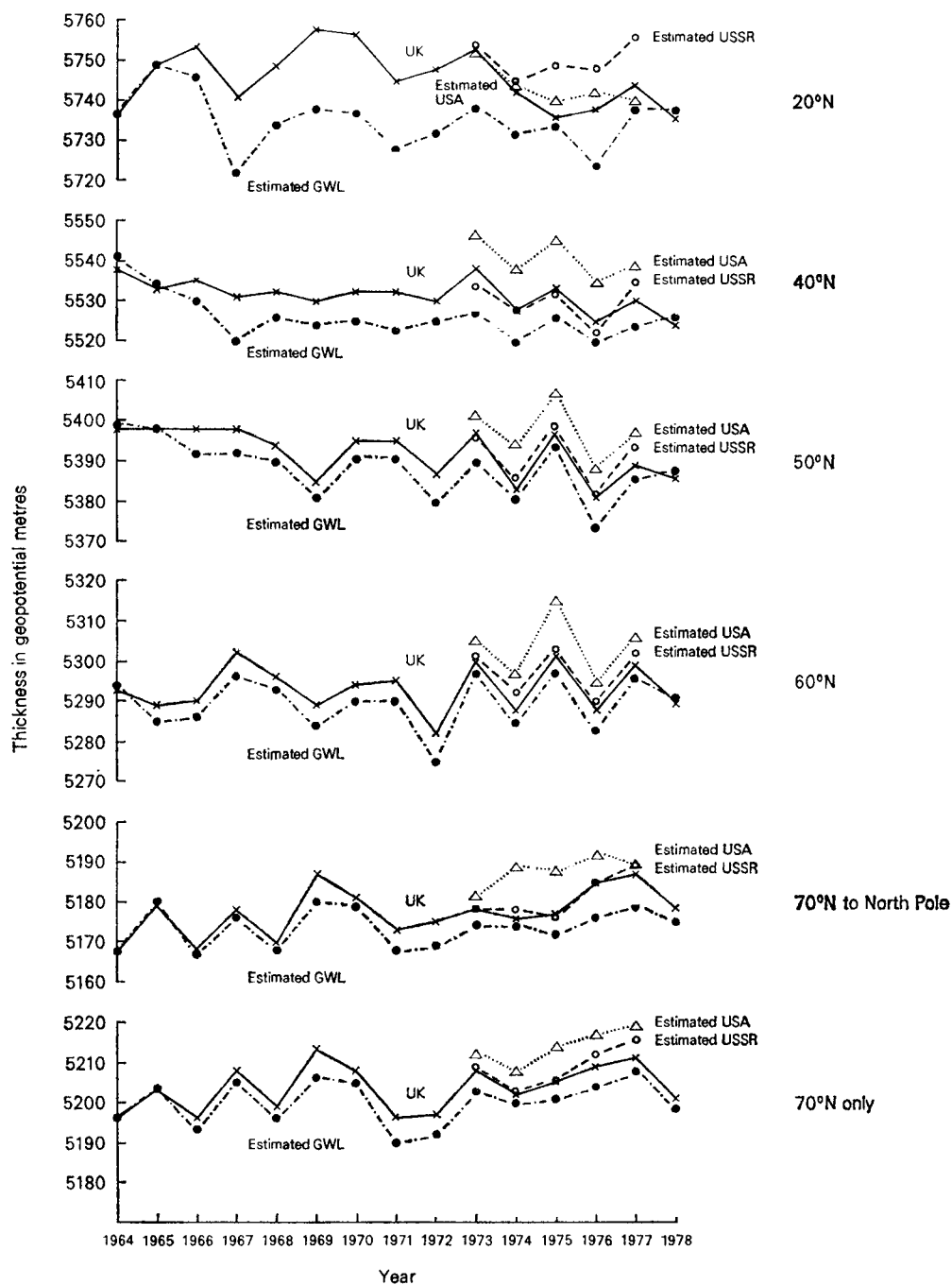
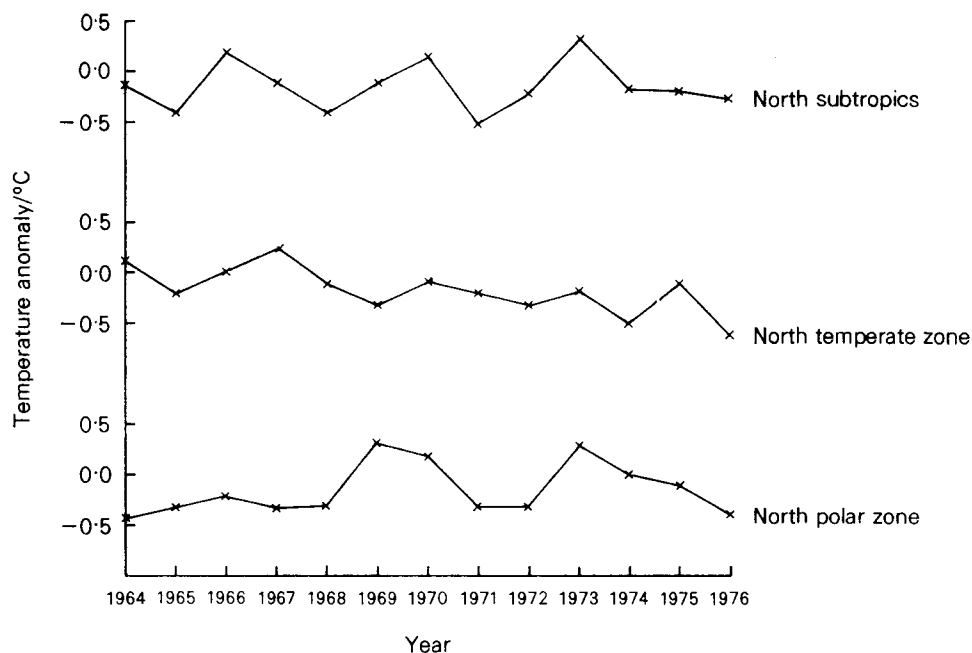


Figure 6(c). Annual mean thickness of the 1000-500 mb layer.



Figures 6(d). Annual mean temperature anomalies of the surface to 100 mb layer (K), after Angell and Korshover (1978).

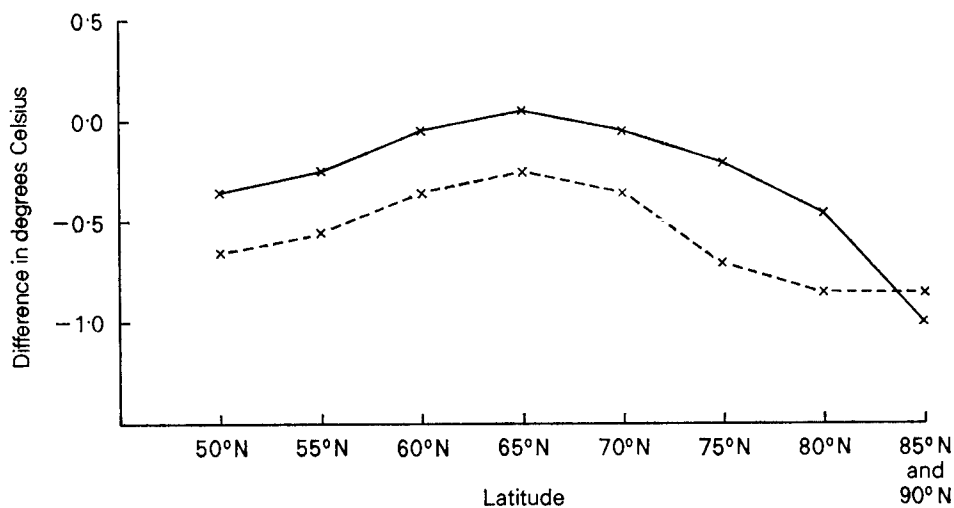


Figure 6(e). Annual mean temperature of the 1000–500 mb layer (K), 1973 minus 1949.  
x - - x Dronin (1974) X — X UK grid-point data (used by Painting (1977)).

#### 4. Causes of the disagreements

##### 4.1 *General considerations*

The following must be considered as possible general reasons for the discrepancies between the different nations' analyses.

(i) The data bases of the daily analyses may have differed. Data from different radiosonde stations may have reached different analysis centres. Moreover different quantities of satellite and aircraft data may have been employed.

(ii) Different radiation corrections may have been applied to the same radiosonde data at the different analysis centres.

(iii) Earlier daily analyses may have been subjective; the later ones are more likely to have been made objectively but with some subjective intervention including the rejection or amendment of data considered to be in error.

(iv) Objective analysis schemes have certainly differed between analysis centres and have probably been amended progressively. If the first-guess field used in the analysis routine was a forecast derived from earlier fields, it will have depended on the model used. The horizontal resolution, interpolation scheme, and the incorporation of dynamical constraints (connecting e.g. heights and winds) will have varied from model to model.

Rinne and Frisk (1979) have computed the development of the analysis error of the 500 mb geopotential during 1946 to 1969, assuming optimum interpolation of the data from the radiosonde network with specified realistic observation error and estimated first-guess forecast-field error. Their areas of maximum estimated error are remarkably consistent with the areas where marked disagreements between analyses appear in Figures 1–3.

(v) Estimates of the effective mean sea level pressure for mountainous areas will have depended on the model or reducing equation used. Absolute geopotentials at pressure surfaces will have been affected more than thicknesses.

(vi) Annual cycles of analysis differences over mid-Pacific will have been caused by the diverse effects on the different analysis schemes of the real annual cycle of atmospheric geopotential fields. Over land, annual cycles of differences are more likely to have resulted from differing treatments of radiation corrections, which have an annual cycle.

(vii) The high UK values over the North Pole are thought to have resulted from a tendency, reflected in the background fields, for the model to forecast excessively high values there. The appearance of this discrepancy in 1975, when analyses from the model became the basis of the grid-point values, is consistent with this explanation. The slightly high UK values, relative to GWL, in recent years over eastern North America may have resulted from erroneous application of radiation corrections in the United Kingdom.

##### 4.2 *Specific example*

The worst case of disagreement is a difference of 150 gpm between GWL and USSR 500 mb geopotentials for 40°N 170°W on the monthly chart for January 1977. This point is one of the furthest from a radiosonde station, and is in a region of strong geopotential gradients especially in winter. It is therefore highly susceptible to variations in the method of analysis. The daily analyses made by the Deutscher Wetterdienst and the USSR in this area were examined, and the findings are given in Table I. This shows that the USSR analyses have stronger gradients around Aleutian lows of similar or lesser



**Table 1.** 500 mb geopotentials at 40°N, 170°W during January 1977. Values are in decageopotential metres and are for 00 GMT.

Date	Geopotential	Grosswetterlagen analysis Local synoptic pattern	Geopotential	USSR analysis Local synoptic pattern	Comments (see key below)
1	534	Flow from WSW.	549	Flow from SW.	(A)
2	533	Flow from SW.	542	Flow from SW.	(A)
3	531	Flow from SW.	531	Flow from SW.	(A) USSR trough further west.
4	519	Troughing. Flow from W.	536	Flow from SW.	(A)
5	523	Flow from SW.	537	Troughing. Flow from SW.	(A) USSR trough deeper.
6	520	Troughing. Flow from SW.	517	Flow from WSW.	
7	534	Flow from W.	533	Troughing. Flow from SW.	
8	523	Troughing. Flow from SW.	524	Troughing. Flow from SW.	(A) USSR had extra low at 40°N 160°E.
9	519	Troughing. Flow from WSW.	530	Troughing. almost col. Flow from SW.	
10	521	Flow from SW.	532	Flow from SW.	(A) USSR low further north.
11	510	Cyclonic. Flow from SW.	536	Troughing. Flow from W.	(A) A 20° difference in wind direction at station 70316 between analyses.
12	519	Flow from WSW.	535	Flow from SW.	USSR low shallower and further north.
13	520	Flow from WNW.	545	Flow from WSW.	(B)
14	520	Flow from WNW.	543	Flow from W.	(A)
15	522	Flow from WSW.	535	Flow from SW.	(A)
16	515	Slight troughing. Flow from WSW.	526	Flow from WSW.	(A) USSR low shallower with stronger gradients round it.
17	514	Flow from WSW.	531	Flow from SW.	
18	515	Cyclonic. Flow from W.	539	Flow from WSW.	(B)
19	527	Trough to NE. Flow from WNW.	539	Troughing. Flow from SW.	(A) USSR had strong flow from SW where GWL had flat trough 45°–50°N 165°–170°W.
20	526	Trough slightly to E. Flow from WNW.	528	Trough slightly to W. Flow from WSW.	
21	526	Flow from W.	524	Shallow trough to W. Flow from WSW.	
22	527	Flow from SW.	544	Flow from WSW.	(A)
23	512	Sharp troughing. Flow from SW.	539	Flow from SW.	(B) USSR had strong flow from SW where GWL had trough.
24	512	Cyclonic. Flow from W.	549	Trough to W. Flow from SW.	(D) USSR had strong flow from SW where GWL had low. USSR had extra low 30°N 175°W.
25	530	Trough to W. Flow from WSW.	542	Trough to E. Flow from W.	(C)
26	517	Cyclonic. Flow from SW.	525	Cyclonic. Flow from SW.	USSR low further NW, deeper, with much stronger gradients round it.
27	510	Cyclonic. Flow from SW.	520	Cyclonic. Flow from SW.	USSR low further NW with slightly stronger gradients round it.
28	521	Cyclonic. Flow from NW.	532	Cyclonic. Flow from WNW.	USSR low further north with slightly stronger gradients round it.
29	537	Ridge to W. Flow from WNW.	549	Flow from SW.	(C)
30	542	Ridge to E. Flow from WSW.	534	Ridge to W. Flow from WNW.	USSR low slightly deeper and further south.
31	517	Cyclonic. Flow from SW.	535	Cyclonic. Flow from SW.	USSR low shallower and further north-west.
Average	522.5	(monthly chart 522)	534.9	(monthly chart 537.5)	

**Key to Comments:**  
 (A) USSR had stronger gradients around Aleutian low of similar depth, rendering peripheral values higher.  
 (B) USSR low shallower and further north with stronger gradients round it.  
 (C) USSR flow from SW extended further north into Grosswetterlagen low zone.  
 (D) Some questionable data.

depth than the GWL analyses, resulting in greater geopotentials in mid-Pacific. The cross-sectional shape of a depression on an analysis will be very sensitive to the incorporation of dynamical constraints and to the grid-resolution of the model: an observation of calm at the centre of a depression, coupled with a geostrophic constraint, will tend to cause the analysed geopotential or pressure at surrounding grid points to be nearly as low as the central value. Some analysts may also apply the same idea subjectively, whereas others (in this case apparently mainly in the USSR) may conceive depressions to have very small centres surrounded immediately by tight gradients. The problem would not arise if data were plentiful.

Figures 7(a) and (b) are the GWL and USSR analyses for 00 GMT on 24 January 1977, for which the analysed geopotential at 40°N 170°W (the large dot) differs by 370 gpm. The above-mentioned tendency for tighter gradients round USSR lows combines with some questionable data, and a general sparsity of information, to make the analyses totally different.

### *5. Recommendations*

It has been shown that the combined uncertainties in data and in analytic techniques make it impossible to reach conclusions on recent trends of the mean temperature of the northern hemisphere, obtained from grid-point data sets. For the tropics and the southern hemisphere, where data are much sparser, the uncertainties must be much greater.

Two courses of remedial action present themselves. They could be followed simultaneously under the aegis of the World Climate Programme.

Firstly, it is proposed that a set of upper-air stations be selected for global climate monitoring. The upper-air data from these stations should be subjected to meticulous quality control, taking account of past changes of instruments and of observing procedure. The stations should be selected on the grounds of continuity of record and of good data quality. They should be as evenly spaced as possible and preferably juxtaposed, or nearly so, to surface stations having a long reliable record, the data from which should also be meticulously quality-controlled. The author has begun to make such a selection.

Secondly, since the best sets of fixed-station data cannot give a complete picture of world climatic change because of the gaps over the oceans, the above proposal should be complemented by the creation of an internationally agreed backdated grid-point data set covering upper-air and surface data for the atmosphere, and sea-surface temperature. The observations used to create the data set should be as complete as possible, and thoroughly quality-controlled, and the analysis procedure should take into account the structure of atmospheric fields in data-sparse areas.

The practical difficulties in agreeing on and creating such a data set may be formidable, but there appears to be no other way of obtaining a complete representation of climatic change.

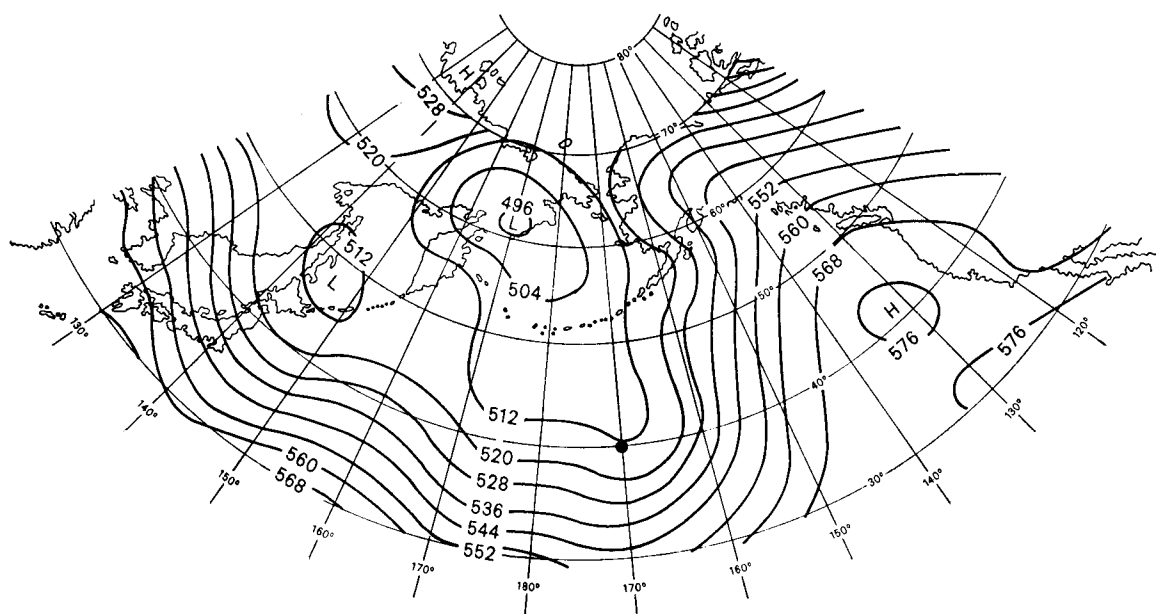


Figure 7(a). Deutscher Wetterdienst (GWL) 500 mb geopotential analysis for the North Pacific, 00 GMT, 24 January 1977. Isopleths at intervals of 8 decageopotential metres.

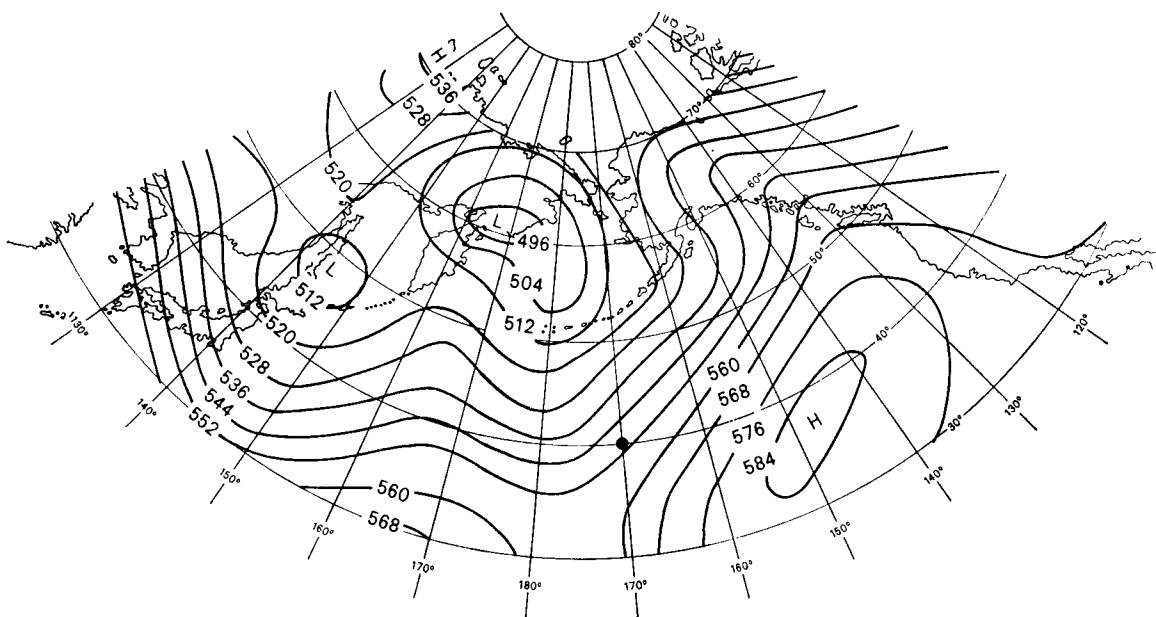


Figure 7(b). USSR 500 mb geopotential analysis for the North Pacific, 00 GMT, 24 January 1977. Isopleths at intervals of 8 decageopotential metres.

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## A revised rainfall series for Spalding, Lincolnshire

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## Summary

Two possible discontinuities in the rainfall series for Spalding published by Craddock and Wales-Smith (1977) are identified. Their removal reduces the considerable long-term variations in rainfall present in the original series, and secures much better agreement with other long rainfall records.

## 1. Introduction

Craddock and Wales-Smith (1977) produced a series of monthly rainfall totals representative of a site at Pode Hole, near Spalding in Lincolnshire, back to 1726. This record produced some marked changes in rainfall over the last 250 years which seem rather out of line with most other indications. This promoted an investigation into the Spalding record to see if evidence from other rain-gauges could help to resolve the differences.

## 2. Comparison with other long rainfall series

In Figure 1 decadal means of annual rainfall for the Spalding series are compared with those for three other rainfall records which go back into the first half of the eighteenth century. These are:

(i) The well-known series for England and Wales, first published by Nicholas and Glasspoole (1932) and since maintained by the Meteorological Office.

(ii) The record for Kew compiled by Wales-Smith (1980).

(iii) A series for Hoofddorp in The Netherlands, derived from data published by Labrijn (1946).

The data for Kew and Hoofddorp indicate a general absence of long-period trends, but the England and Wales series shows a marked increase in rainfall, while the graph for Spalding displays a strikingly concave form. The low rainfall totals indicated by the England and Wales series in the eighteenth century may not be real. During this period the number of gauges used was small, and their sites were unorthodox, with a preponderance of faults leading to an underestimate of the rainfall likely to be recorded by a

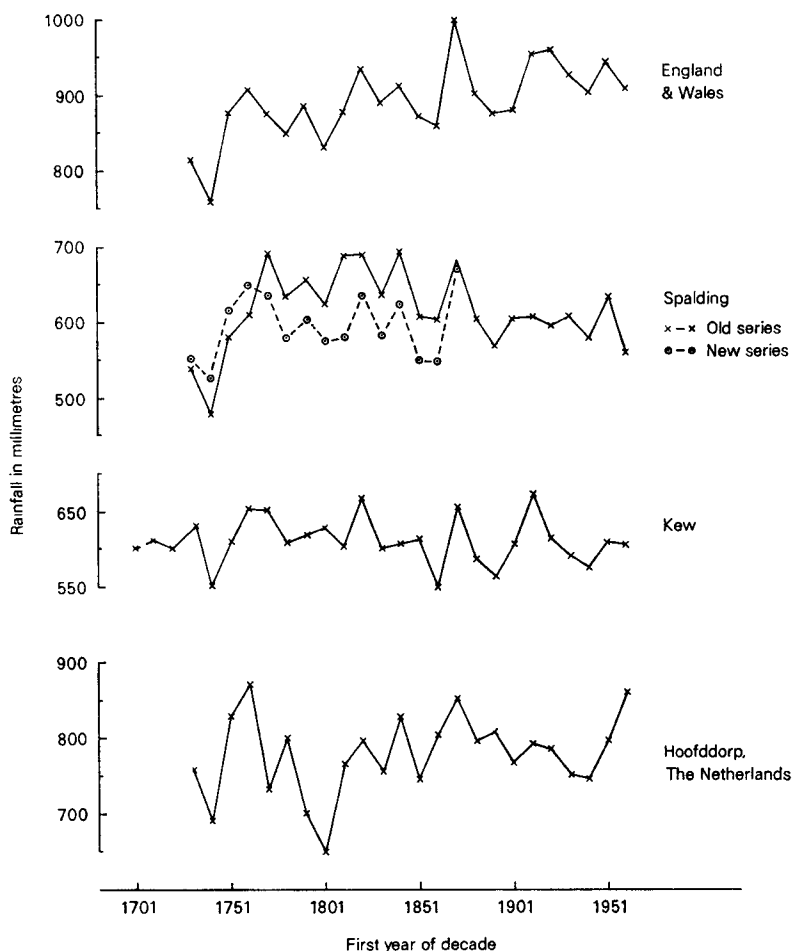


Figure 1. Decadal means of annual rainfall for four long rainfall series.

modern gauge. In 1820 the number of stations available reached 17, and for the first time mapping techniques could be used to calculate the areal rainfall. Only from this date can the England and Wales series be regarded as reliable. The concave nature of the Spalding record may be largely attributed to a period between about 1770 and 1870, which, in comparison with the other records, was too wet.

A closer examination of the period since about 1850 is made in Figure 2, where decadal means of annual rainfall at Spalding are compared with decadal means for other stations in eastern England. These latter are mainly unpublished series made homogeneous by the author. Also included is a series for the location 53°N 0°W, very close to Spalding and not very far from Mansfield, obtained from data

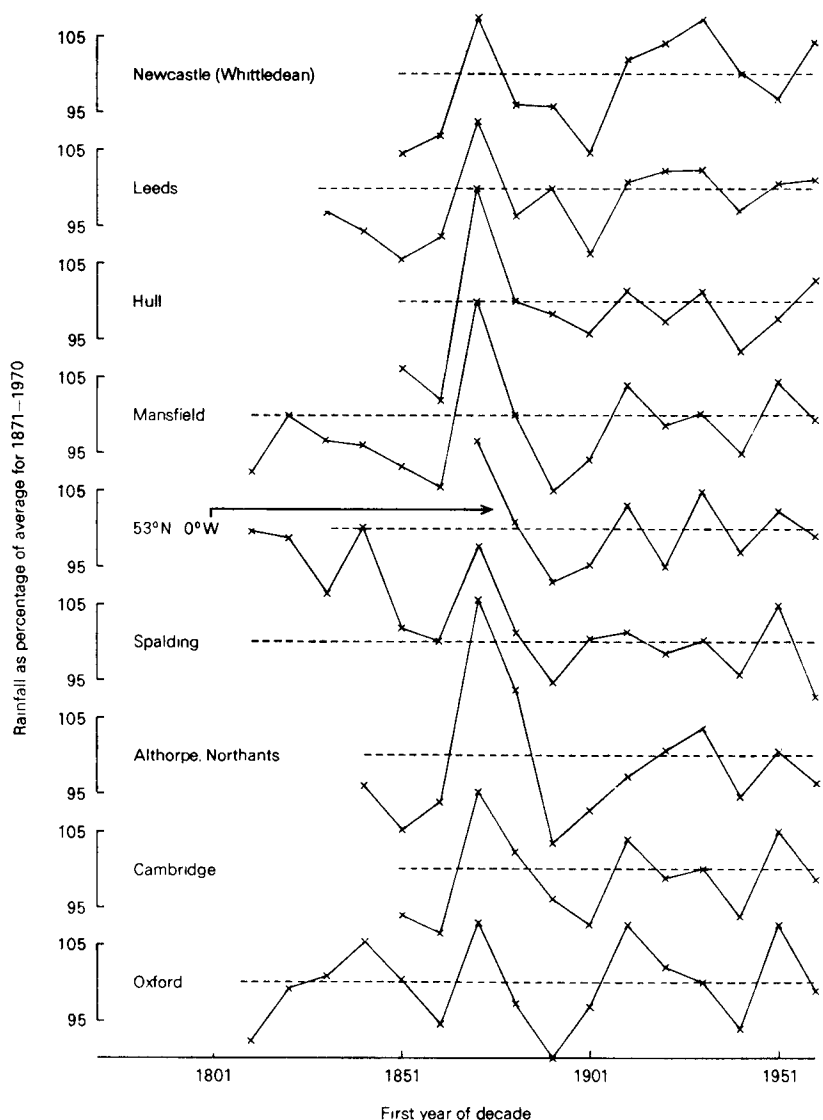


Figure 2. Decadal means of annual rainfall for stations in eastern England.

derived from maps of percentage of average rainfall which were published by the Royal Meteorological Society (1926) for the years 1868–1923, and by the Meteorological Office in *British Rainfall* thereafter. From 1871 onwards the time series for Mansfield, Spalding, and 53°N 0°W are in very close agreement. Before 1870, however, it is clear that the values for Spalding are too high, and there is evidently a discontinuity in the Spalding record around 1870.

### 3. Examination of the period around 1870

The period from 1864 to 1879 was examined in more detail by comparing annual rainfalls at Pode Hole with those meaned over five surrounding gauges located at a distance of about 25–30 km. The data were obtained from the ‘10-year’ books held in the Meteorological Office. A considerable decrease in the catch at Pode Hole relative to that at the other gauges is indicated.

The rim of the gauge at Pode Hole was at ground level up to 1871, at 3 inches from 1872 until 1890, and at 1 ft or 15 inches thereafter. The change from 3 inches to 1 ft does not appear to have made much difference to the catch, but it is possible that the change in 1871 did, perhaps because of in-splashing when the rim was at ground level. The catch at Pode Hole expressed in terms of that meaned over the five neighbouring gauges was 111·5 per cent from 1865 to 1871, and 101·2 per cent from 1872 to 1879. This suggests that the gauge at Pode Hole shared a similar exposure to its neighbours from 1872 onwards, but caught 10 per cent more before that date. A correction factor of around 10 per cent is consistent with what might have been deduced from Figure 2, where a reduction of the decadal means at Pode Hole by 10 per cent prior to the 1870s would secure a good agreement with the other records.

### 4. Examination of the period 1800–70

The record produced by Craddock and Wales-Smith for the period 1800–70 is based mainly on data from South Kyme (Lincs.) from 1800 to 1828, and from Pode Hole thereafter. The South Kyme record extends to 1868, but other data examined were for Witham on the Hill (1821–96) and Boston (1826–72), both within 30 km of Pode Hole, plus more distant stations at Derby (1809–35) and Welbeck Abbey, Notts. (1807–76). The subjective judgement from these observations was that the Pode Hole record was good, but that South Kyme caught too much rain between 1814 and 1816 and too little between 1847 and 1856. The catch at Witham also seemed deficient after 1860.

The Pode Hole record was therefore regarded as homogeneous between 1829 and 1871. South Kyme was used to extend the series back to 1800, but the ratio between the rainfalls at South Kyme and Pode Hole was determined using overlaps for both stations with Witham on the Hill. For Pode Hole the period used was 1829–60, while for South Kyme the ratio with Witham was derived from the periods 1821–46 and 1857–60. The high totals received at South Kyme in the years 1814–16 were further reduced by 27 per cent from comparisons with Welbeck Abbey.

### 5. Examination of the period 1726–1800

For the period 1726–1800 the series is based mainly on data from Southwick (Northants.) from 1726 to 1739, and Lyndon (Rutland) from 1737 to 1798.

The nearest check on the Lyndon observations is from the record at Chatsworth, Derbyshire (1761–1813). The ratio between annual totals at the two gauges during the period of overlap was not very steady, but a marked change in the ratio, in the sense of an increase in rainfall at Lyndon, took place around 1770. Thomas Barker wrote to the Royal Society describing the site of his gauge on a wall in 1771. It is possible that after this communication, he moved his gauge on to a lawn, being unaware that

this would affect its catch. If the change is assumed to have taken place around 1771 or 1772, then the change in the ratio Lyndon/Chatsworth between 1761–71 and 1772–1800 indicates a change of 15.7 per cent in the catch at Lyndon.

In order to reduce the Southwick record to that at Lyndon, there are only three years of overlap, with the ratio Lyndon/Southwick = 0.913. At Southwick, Craddock and Wales-Smith assumed an average annual rainfall ( $R$ ) for a standard site of 23.8 inches. This, combined with an estimated oversheltering of 3 per cent gave an expected  $R$  of 23.11 inches. At Lyndon an estimated  $R$  for a standard site of 24.56 inches may be combined with an over-exposure of 15.7 per cent to give an expected  $R$  of 21.24 inches. The ratio of the expected  $R$  (Lyndon/Southwick) is 0.919, almost identical to the ratio of the catches that was actually observed. This finding supports the use of the overlapping ratio to reduce the observations at Southwick to those at Lyndon, and the assumptions leading to the estimated over-exposure of 15.7 per cent in the gauge at Lyndon.

The reduction of observations from Lyndon to South Kyme cannot be made with any degree of confidence, but overlaps with Chatsworth of 1772–1800 for Lyndon and 1800–13 for South Kyme suggest a ratio of unity between rainfall at Lyndon and South Kyme.

## 6. Conclusions

The suggested factors for obtaining a revised rainfall series representative of Spalding are summarized in Table I. They are all uncertain, and by analysing the data in different ways, different results could have been obtained. Nevertheless, the factors presented in Table I are probably just as good as any others, and they do result in a homogeneous series which agrees well with other long-period records. This is illustrated in Figure 1 where the original and suggested series for Spalding are compared with those for England and Wales, Kew, and Hoofddorp. Between the 1770s and the 1860s the old and new series for Spalding are practically parallel, the major differences being the step changes introduced in 1871 and 1771. The first of these is well founded, but the second is open to conjecture. The similarity with the other records available in the eighteenth century implies, however, that the present suggestions are unlikely to be substantially in error. Both authors of the original series for Spalding have agreed to the revisions suggested above.

**Table I.** *Homogeneous rainfall series for Spalding*

Period	Station	Multiplying factor	
		New	Old
1726–39	Southwick	1.019	1.024
1740–71	Lyndon	1.117	1.040
1772–98	Lyndon	0.965	1.040
1799	West Bridgford	1	1
1800–13	South Kyme	0.965	1.030
1814–16	South Kyme	0.705	1.030
1817–25	South Kyme	0.965	1.030
1826	Witham on the Hill	1	1.011
1827–28	South Kyme	0.965	1.030
1829–71	Pode Hole	0.908	1
1872–	Pode Hole	1	1

*Note.* The multiplying factors in the column headed 'New' are those which must be applied to the readings from the various stations in order to produce the revised unified series for Spalding which is now proposed; the factors in the right-hand column are those which were used for this purpose by Craddock and Wales-Smith (1977).



## Acknowledgements

Thanks are due to Messrs B. G. Wales-Smith and J. M. Craddock for constructive discussions during the course of this revision.

## References

- |  |      |   |
|--|------|---|
| Craddock, J. M. and Wales-Smith, B. G. | 1977 | Monthly rainfall totals representing the East Midlands for the years 1726 to 1975. <i>Meteorol Mag</i> , 106, 97–111.   |
| Labrijn, A.                            | 1946 | Het klimaat van Nederland gedurende de laatste twee en een halve eeuw. [The climate of the Netherlands during the last two and a half centuries.] KNMI, De Bilt, <i>Meded en Verh</i> , No. 49.                       |
| Nicholas, F. J. and Glasspoole, J.     | 1932 | General monthly rainfall over England and Wales, 1727 to 1931. <i>Brit Rainf</i> 1931, 299–306.   |
| Royal Meteorological Society           | 1926 | Rainfall Atlas of the British Isles, London.  |
| Wales-Smith, B. G.                     | 1980 | Revised monthly and annual totals of rainfall representative of Kew, Surrey for 1697 to 1870 and an update analysis for 1697 to 1976. To be published as <i>Meteorological Office Hydrological Memorandum</i> No. 43. |

## Supplementary note on 'The northerly gales of 11–12 January 1978' (*Meteorological Magazine*, 108, 1979, 135–146)

### *Estimates of wind above the boundary layer*

Mr C. L. Hawson of the Special Investigations Branch of the Meteorological Office has given me details of wind speeds recorded at the IBA television mast at Belmont, Lincs. (53°20'N, 00°08'W) on 11–12 January 1978. The anemometer, the property of the Central Electricity Research Laboratories, is mounted on top of the mast at 1275 ft above ground and 1686 ft above mean sea level, and the speeds recorded should be close to the wind speeds in the free air above the boundary layer. No regular instrument checks are carried out, but a number of indirect comparisons have not revealed any consistent error. Winds are measured over 3-minute intervals, and averaging 20 readings provides an hourly mean. The maximum hourly mean speed recorded on 11 January was 65 knots (1400–1500 GMT) and the maximum 3-minute wind speed was 70 knots (1500–1600 GMT).

A trajectory constructed for the air which at 1500 GMT was over Belmont showed no curvature and a geostrophic wind of 74 knots, only a little greater than the Belmont 3-minute wind speed. In some other areas (e.g. north-west of London in the evening) the geostrophic wind was estimated as 90–100 knots, with trajectory either straight or perhaps with slight anticyclonic curvature, implying a wind speed in the free air of about 100 knots. However, these estimates of geostrophic wind are based on synoptic-chart measurements over distances of 30–60 n mile normal to the isobars, and this averaging distance may not be sufficient to exclude appreciable errors. Moreover a larger uncertainty arises in estimating the trajectory and hence its curvature. Broad-scale features of the sea-level pressure charts suggest an overriding tendency to cyclonic curvature, even though in an area 130 n mile square the isobars were straight and unchanging in direction over several hours. In addition, the curvature 'correction' is proportionately much larger for very strong winds than for moderate speeds.

After consideration of these uncertainties there seems little doubt that the hourly mean wind speed in the free air reached 75–80 knots in some places. It may well be that, in certain areas and for limited periods, higher speeds were reached, but of this we have no direct evidence.

The maximum wind aloft is of interest here primarily in its possible relationship to ground-level gust speeds. The Belmont data at 1275 ft may be compared with the maximum gusts recorded at five anemograph stations all within 20 n mile.

Time GMT	13	14	15	16	17	18	19	20	
Belmont 3 min (max.) (knots)	64	69	70	68	67	65	61		
Belmont 60 min mean (knots)	57	65	64	64	64	62	57		
Max gust (knots)									Time
Scampton	.	.	.	52	.	.	.		(1639)
Waddington	.	.	.	57	.	.	.		(1645)
Cranwell	.	.	.	.	61	.	.		(1739)
Coningsby	.	.	.	.	.	55	.		(1821)
Binbrook	.	.	.	.	.	.	57		(1921)

Mr Hawson also points out that an isallobaric wind theory ascribed to Ertel, and discussed in Panofsky's 'Introduction to dynamic meteorology'\*, would on this occasion have produced a vector opposed to the geostrophic wind. This would imply a true wind appreciably less than the geostrophic or gradient wind, in contrast to the theory of Brunt and Douglas which, if tenable, would imply winds of up to 125 knots. However, Panofsky states that observational evidence is inconclusive regarding the relative merits of the two solutions. Although in some circumstances one or the other isallobaric wind construction may seem to apply, neither can be relied on even in so well-marked a case as that of 11–12 January 1978.

E. G. E. King

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\* Panofsky, H. [A.]. Introduction to dynamic meteorology. Pennsylvania State University, 1956.

### Meteorological Office Scientific Paper No. 38 (The Psychrometer Coefficient of the Wet-bulb Thermometers used in the Meteorological Office Large Thermometer Screen, by C. K. Folland, B.Sc. London, HMSO, 1977)—notification of misprints.

A number of mostly minor misprints have been noted by the author and his colleagues in the above-mentioned Scientific Paper as printed; these represent errors only in the paper as published and not in the work, calculations or conclusions. The four which may be significantly misleading are as follows:

(1). The equations for  $(h_c)_{eyl}$  and  $(h_c)_{sph}$  near the bottom of p. 22 and at the top of p. 23 respectively should read

$$(h_c)_{eyl} = 74 \left( \frac{V}{r} \right)^{\frac{1}{2}} \text{ W m}^{-2} \text{ K}^{-1}$$

and

$$(h_c)_{\text{sph}} = \frac{12}{R} + 54 \left( \frac{V}{R} \right)^{\frac{1}{2}} \text{ W m}^{-2} \text{ K}^{-1}.$$

The parent equation on p. 22 is correct.

(2). On p. 27, line 3, 'Figure 6' should read 'Figure 16'.

(3). The numerical values for  $k'_2$  and  $k''$  given half-way down p. 31 should be transposed, i.e.

$$k'_2 = 10.9 \text{ W m}^{-1} \text{ K}^{-1}$$

$$k'' = 18.4 \text{ W m}^{-1} \text{ K}^{-1}.$$

(4). On p. 35, in the caption to Figure 21, '58 mm' should read '68 mm'.

Additional errors of less importance are:

(5). On p. 3, Table I, third column, '1.10' should read '1.00'.

(6). On p. 6, Figure 2, the letters 'A' and 'B' should be inserted to identify (I) the second from top curve and (II) the bottom curve respectively.

(7). On p. 9, fourth line from bottom, after 'rate' insert 'of change with temperature'.

(8). On p. 21, line after equation (13), 'L' should be added in front of  $h_v \beta_w$ .

(9). On p. 24, 6 lines from bottom, 'Figure 3' should be 'Figure 6'.

(10). On p. 29, two-thirds of the way down the page, the text should read ' $h_c$  and  $h_r$  are evaluated for  $r_d$  or  $r$  as appropriate (a single prime represents the upper stem lower section and a double prime the upper stem upper section).'

(11). On p. 30, equation (20) should read

$$\left( \frac{dQ}{dt} \right)_{x=0} = kB \left( \frac{d\theta}{dx} \right)_{x=0} = \frac{kBm (T_w'' - T_w')}{\sinh mL}.$$

## Notes and news

### Meteorological Research Committee and Advisory Committee on Meteorology for Scotland

The recent White Paper entitled 'Report on Non-Departmental Public Bodies (Cmnd. 7797, HMSO, January 1980) states that 'the Meteorological Committee will absorb the activities of the Advisory Committee for Meteorology in Scotland, and the Meteorological Research Committee; savings are estimated at £0.003 m'.

## Obituary

We regret to record the death on 19 December 1979 of Mr W. F. McKnight, Higher Scientific Officer. William Fairlie McKnight joined the Office as an Assistant in December 1951 and gained promotion to the forecasting grade of Assistant Experimental Officer in 1957. He served at a variety of outstations, both at home and overseas, in his early years in the Office before being posted in 1967 to Glasgow Weather Centre where he was still serving at the time of his death. He was noted for his keen interest in sports and social activities. In his younger days he played Rugby for Kilmarnock in the 1st XV and cricket in the 1st XI, and he was later made honorary life president of Kilmarnock Cricket Club.

We regret to record the death on 25 December 1979 of Mr R. Mills, Scientific Officer. Richard Mills joined the Office as an Assistant in June 1947 after REME service during the war and was promoted to Senior Scientific Assistant in 1964. He served at many outstations at home and overseas in his early career, and also worked for a couple of years in the Communications Section (M.O.5a) at Dunstable and Bracknell. Since 1967 he had been Officer-in-charge of the office at Blackpool Airport. He was a keen and expert gardener and a student of plant-raising and cultivation methods; some of his colleagues may remember his successful efforts in this line while he was stationed in Egypt.

We regret to record the death on 15 January 1980 of Mr J. H. McCabe, Scientific Officer. John Hunter McCabe joined the Office as Scientific Assistant in 1947 and was promoted to Senior Scientific Assistant in 1968. The first 20 years of his service were spent at Leuchars and Lerwick Observatory. He was posted to ATCC Preston in 1967, and was then transferred to Manchester Airport in 1978 when the Preston office closed. He was a keen golfer, and was a founder member of the Barton Hall Golf Society and a member of the Preston Golf Club.

## Correction

*Meteorological Magazine*, 109, 1980, 74. The first reference on this page should read

Foot, J. S. 1972 Snow accretion on overhead powerlines. *London Weather Centre Memorandum* No. 23.

This memorandum is not unpublished, as previously stated. It can be obtained for £1.50, plus postage, from

**The Principal Meteorological Officer,  
London Weather Centre,  
284-286 High Holborn,  
London WC1V 7HX**

**or**

**The Director-General,  
Meteorological Office Met 0 7a,  
London Road,  
Bracknell,  
Berkshire RG12 2SZ.**



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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by

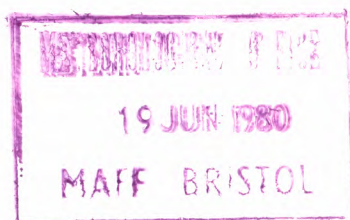
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 4/80

Annual subscription £21.18 including postage  
ISBN 0 11 722061 2  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

June 1980

Met.O. 931 No. 1295 Vol. 109





# THE METEOROLOGICAL MAGAZINE

No. 1295, June 1980, Vol. 109

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## Data processing in the Meteorological Office Short-period Weather Forecasting Pilot Project

By C. G. Collier

(Meteorological Office Radar Research Laboratory, RSRE, Malvern)

### Summary

The Meteorological Office Short-period Weather Forecasting Pilot Project, which began in 1978, required the development of techniques for processing radar and satellite imagery. In this paper the data-processing system is described which enables data from several radars to be combined with data from Meteosat to give the mesoscale pattern of precipitation. The resulting data are presented within a minicomputer environment to small teams of research meteorologists and forecasters. The system is structured as a distributed processing minicomputer-network mostly using dedicated communications lines rented from the Post Office. Instantaneous fields of precipitation, and rainfall totals integrated for short time-periods over areas defined by users, are distributed from each radar site to a number of Meteorological Office and Water Authority users, who are at present assessing the usefulness of the data for real-time operations. The software modules which are used in this system are discussed and the data archives which are being created are described.

### 1. Introduction

The aims of the Meteorological Office Short-period Weather Forecasting Pilot Project have been listed by Browning (1977) as:

- (i) To establish and operate facilities to provide mesoscale observational fields of cloud and precipitation (albeit at first over only a part of the country in the case of some of the data), and, in the light of practical experience, to optimize the accuracy, reliability, and the clarity and timeliness of presentation, of the data.
- (ii) To exploit these data to improve our understanding of the structure, mechanism, evolution, and predictability of precipitation and associated wind systems.
- (iii) To develop simple analytical procedures to optimize the use of these data for the provision of improved forecasts of precipitation and wind (initially over a period of a few hours, but with a view to extending the period of improved forecasts up to 6 h or longer).
- (iv) To assess from practical experience the utility of the actual and forecast fields of precipitation to users.
- (v) To assess the desirability, and most cost-effective way, of extending the mesoscale observational network and forecasting techniques.

The Project is primarily concerned with the measurement and forecasting of surface precipitation. Although data from several levels in the atmosphere will be collected, these are intended solely to improve the estimates of the precipitation reaching the ground.

Mesoscale observations of precipitation and cloud have been greatly improved by recent advances in real-time radar data processing and satellite imagery. Work on quantitative rainfall measurement by radar (for a review see Wilson and Brandes, 1979), together with the considerable work reported in the literature on the use of radar for general weather surveillance and analysis (for a review see Browning, 1978), provided the impetus for the use of radar as a key element in the mesoscale observational system. Work over several years at Malvern (Taylor and Browning, 1974) has led to the development of digital methods whereby data from several radars, equipped with on-site minicomputers, can be transmitted to a central location and composited automatically to give a map of the precipitation distribution over a large area. Moreover, the advent of the geostationary weather satellite, *Meteosat*, providing half-hourly cloud imagery over an area including the United Kingdom, has also made the use of satellite cloud data for very-short-period weather forecasting a realistic proposition.

Most of the work to date in the Pilot Project has been aimed at achieving aim (i) above. This has involved establishing an extensive data communications system linking a number of minicomputers providing data from a number of radars and from *Meteosat*. Software systems have been prepared in order to accomplish both real-time and off-line data processing. Detailed descriptions of these systems will appear elsewhere. The purpose of this paper is to describe briefly the data-processing system as a whole, emphasizing how data will be provided to accomplish the other aims of the Project specified above.

## 2. The Pilot Project data network

### 2.1 Principal sources of data

The radar network now being used consists of four radars (Figure 1). Details of the actual radar hardware are given by Ball *et al.* (1979b), and in several internal Met O RRL reports. The radars at Camborne (Cornwall) and Upavon (Wiltshire) are old Plessey 43S radars (10 cm wavelength, 2° beamwidth) sited in a non-optimum manner, and therefore give somewhat limited coverage compared with the other two radars. The radar at Clee Hill (Shropshire) is a Plessey 43C radar (5.6 cm wavelength, 1° beamwidth), which has an horizon at or below 0° in virtually all directions. The 43C is the radar that was operated until recently at Llandegla as part of the Dee Weather Radar Project. The radar at Hameldon Hill, near Burnley (Lancashire), is a new Plessey 45C radar (5.6 cm wavelength, 1° beamwidth). The Hameldon Hill radar forms part of a separate project, known as the North West Radar Project (Collier *et al.*, 1980) (see Appendix A). The Camborne, Upavon and Clee Hill sites are manned (in the case of Clee Hill, Civil Aviation Authority personnel are close by, but no Meteorological Office staff are on the site). The Hameldon Hill radar is completely unmanned, and represents the first such quantitative weather radar in the British Isles. These radars are capable of producing precipitation data at one-minute intervals with a minimum resolution of about 1 km, although resolutions of 5 min and 2 or 5 km are used in the Pilot Project. Personnel at the Met O RRL Malvern, and local technical staff of the Meteorological Office Maintenance Organization (Met O MO) are operating the radars at Camborne, Upavon and Clee Hill 24 hours a day. The Hameldon Hill radar system, on the other hand, will be regarded as operational from about the beginning of 1980, and its maintenance will be undertaken jointly by local (Aughton) Met O MO staff, and staff of the North West Water Authority.

The satellite, *Meteosat*\*, is in a geostationary orbit over the equator, and is capable of providing infra-red (IR) and visible (VIS) cloud imagery at 30 min intervals with a resolution in the IR at 50°N

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\* *Meteosat* 1 ceased operating in November 1979; *Meteosat* 2 is expected to be launched in September 1980.

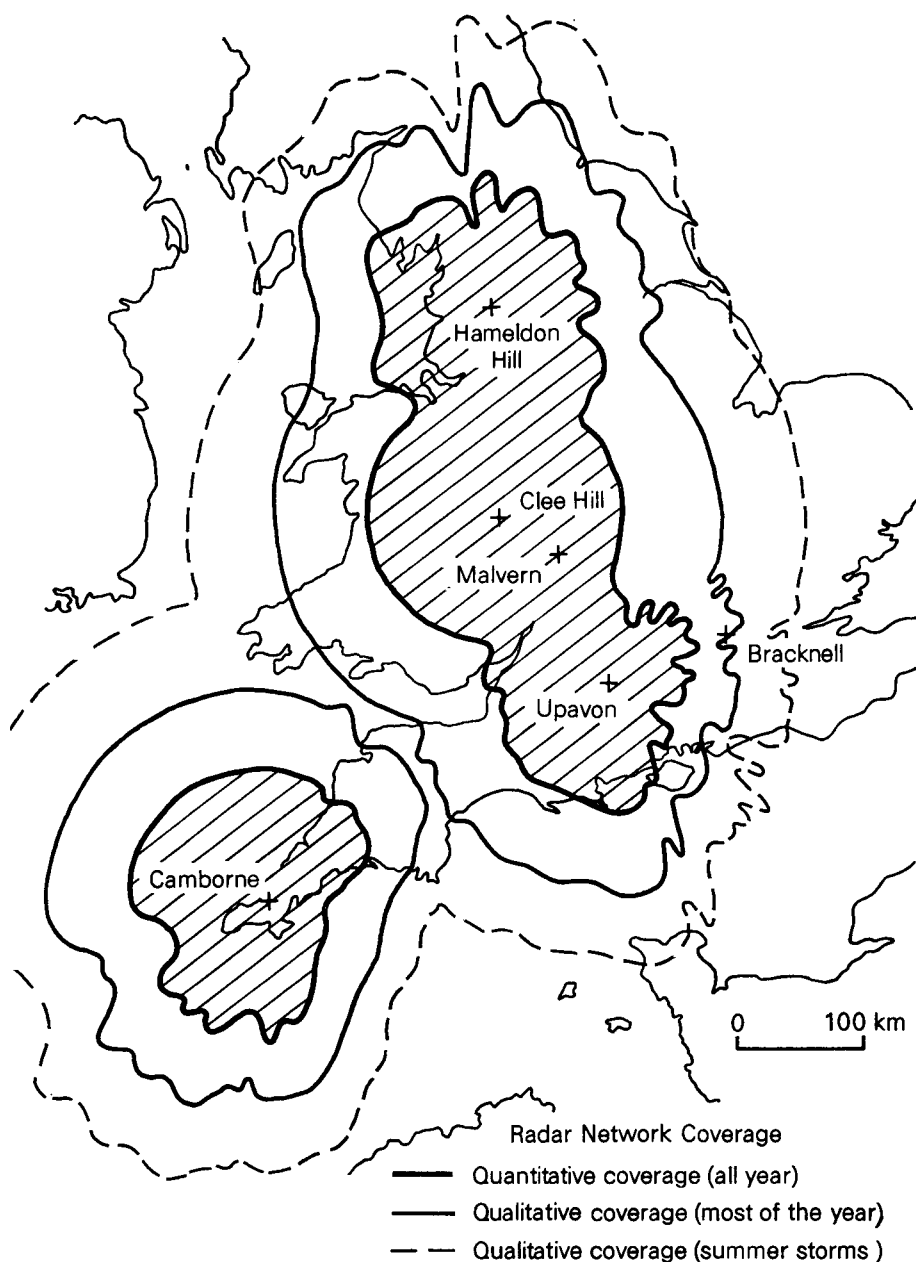


Figure 1. Approximate area within which precipitation can be observed using radars located at Camborne (Cornwall), Upavon (Wiltshire), Clee Hill (Shropshire), and Hameldon Hill (Lancashire). Areas with potentially quantitative coverage are shaded, and the areas of qualitative coverage are enclosed by a broken line.

of 6 km E-W and 12 km N-S (plus some overestimation of the northward extent of cloud because of the oblique angle of view). These data are received at a central ground station in Darmstadt (Federal Republic of Germany) for processing, archiving and redistribution to real-time users via Meteosat

itself (Morgan, 1978). There are two types of user receiving station: primary and secondary. The Primary Data User Station (PDUS) receives high-resolution digital data, and the Secondary Data User Station (SDUS) receives data in analogue format (WEFAX). The Meteorological Office has established a SDUS at Lasham (Hampshire). For a start we are at present making the best possible use of imagery obtained via Lasham after it has been digitized in the Systems Development Branch (Met O 22) of the Meteorological Office at Bracknell.

## 2.2 *The computer network*

Each radar in the network shown in Figure 1 has its own on-site minicomputer, which is used to accept the raw radar data, apply various corrections and output the data in a number of formats to a variety of locations, including a compositing centre in the Met O RRL at Malvern. The software in use at the radar sites is based on work carried out at Malvern by the Royal Signals and Radar Establishment. Further developments and additions are likely to be made to it for several years to come. This software has been developed for use with Digital Equipment Co. (DEC) PDP-11 series minicomputers (Ball *et al.*, 1976, 1979b), which are being used throughout the Pilot Project data-processing system. The radar software has reached a degree of complexity which requires special test facilities, similar to those used by the Atmospheric Environment Service in Canada (Aldcroft, 1976). Specialized diagnostic software has been developed for the speedy identification of faults at the radar sites. This software provides tests for the various computer interfaces used on site, the radar-computer interface unit, and the display system.

Data from individual radars are supplied direct to a number of operational Meteorological Office locations and Water Authority users in a 3-bit format. These data are transmitted via 600/1200 baud private wires leased from the Post Office. The location of these outlets (referred to later as Jasmin outputs) and the radars supplying them, are shown in Figure 2.

The overall computer network, shown in Figure 3, is a distributed computer network having a radial communications system centred on Malvern. This differs from the larger American National Weather Service AFOS (Automation Field Operations and Services) computer network described by Klein (1976), in which communications are effected using a ring system known as a 'multidrop' system. All the communications lines used in the Pilot Project, with the exception of the links from the Hameldon Hill radar, are private wires leased from the Post Office.

In order to provide data of sufficient intensity resolution for compositing and analysis at Malvern, 8-bit (208 intensity levels only are used) radar data are sent to Malvern via a synchronous line network, operating at 2400 baud (Figure 4). (One baud corresponds to a rate of one signal element per second in an equal-length code; it is usually taken as one bit of data per second.) A field of surface precipitation data is transmitted from each radar site every 15 minutes. Satellite data are received at Malvern from Lasham via Bracknell every 30 minutes via a 600/1200 baud private line. Private Post Office lines have error rates in the region of one bit in  $10^6$  bits, although errors often occur in bursts. It was considered that simple error-checking methods would suffice, and the techniques employed are described in Appendix B.

Several computers are located at Malvern (Figure 3). One of them, a PDP11/40, is referred to as the Network computer. Data from the individual radar sites are fed at 15-minute intervals into this computer, which generates the radar composite pictures and records them on magnetic tape. Satellite data are also received into this computer and archived on magnetic tape. The radar composite data are passed in near-real-time to a further PDP11/34 computer known as the Display computer. This computer is employed to reformat and merge the radar composite and satellite data for flexible use by a team



Figure 2. Illustration of the communication-line network connecting various existing (solid line) and planned (dashed line) users to the radars. All the lines shown carry 3-bit data and are land-lines leased from the Post Office and operated at a data rate of 600/1200 baud.

of forecasters at Malvern. The display-computer software, although relatively simple at present, is being enhanced in line with the FRONTIERS program described by Browning (1979).

Development of the radar-site software is carried out at Malvern on a further PDP11/34 computer.

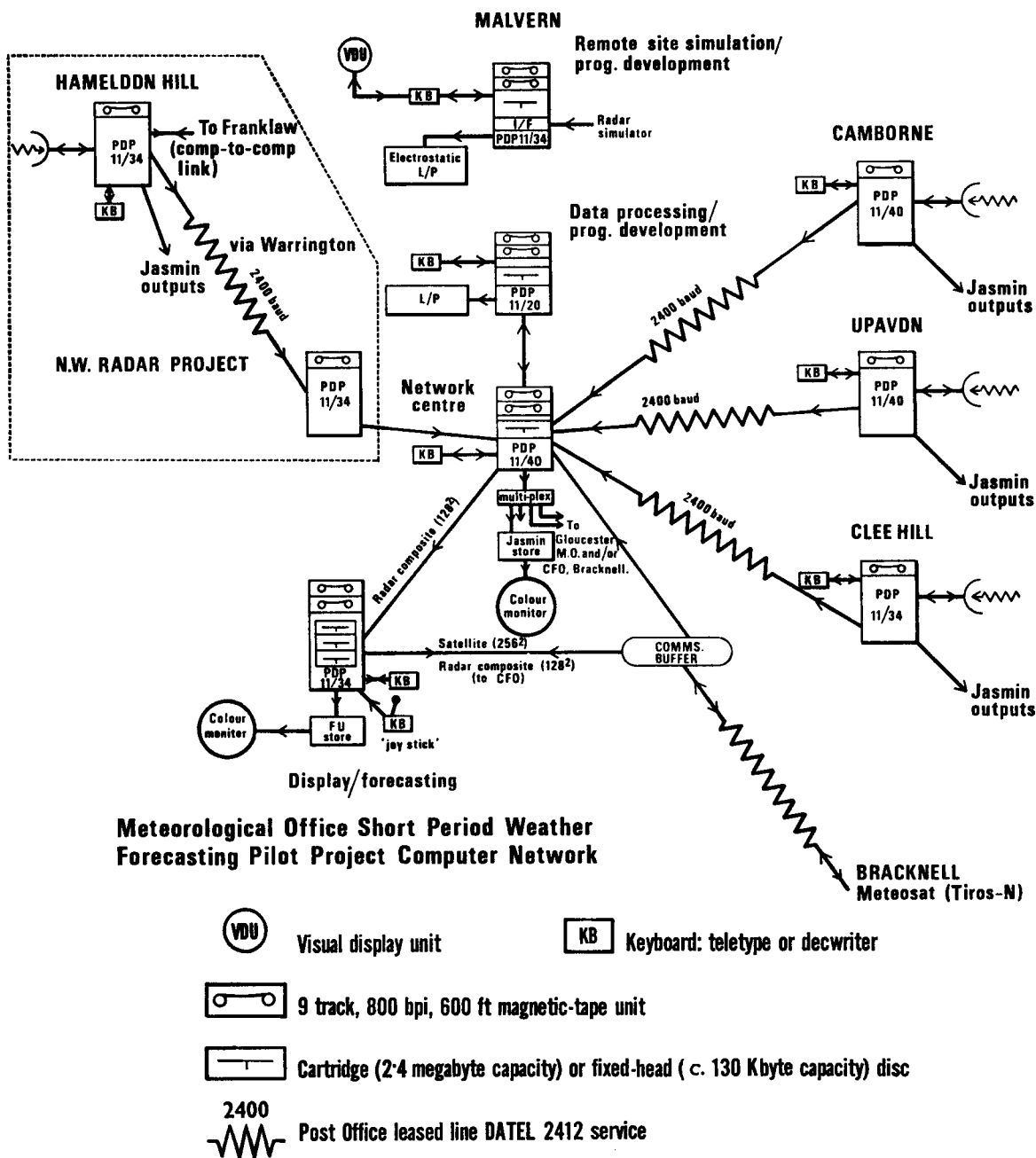


Figure 3. The type, location and interrelation of the minicomputers making up the Pilot Project computer network.

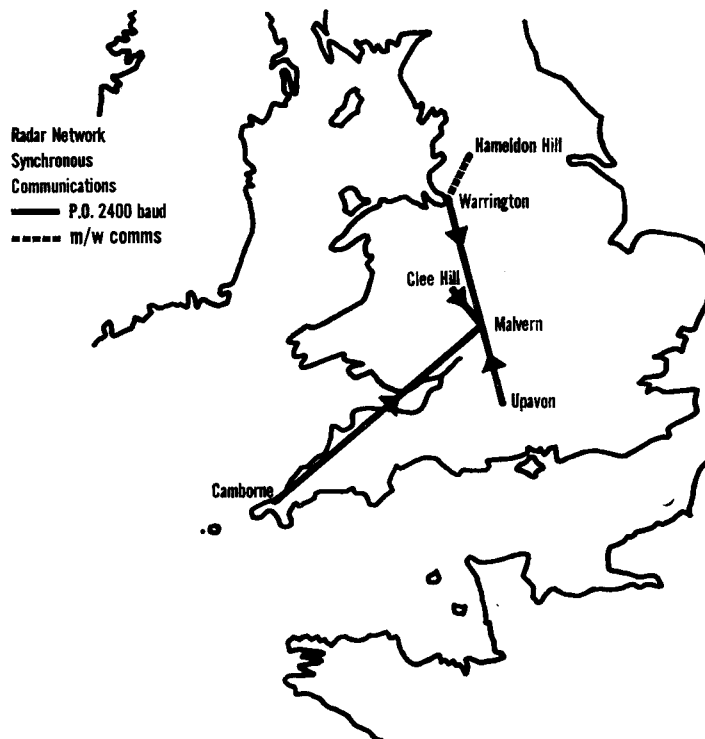


Figure 4. The communications network connecting the radars to the Meteorological Office Radar Research Laboratory at Malvern. All the lines (except the microwave link used in the North West Radar Project) are land-lines leased from the Post Office and operated at a data rate of 2400 baud.

Radar video data and azimuth interrupts may be simulated on this computer with a version of the electronic radar-computer interface used at the radar sites (Ball *et al.*, 1979a), and an analogue-to-digital unit provided with data from a simple clocked electronic signal generator. This facility allows the radar-site software to be developed and tested at Malvern before being passed to the radar sites. Routine off-line data processing, such as the provision of hard copy, is also carried out on this computer, and on a further (PDP11/20) computer. The above computers at Malvern (with the exception of the Network computer) have magnetic tape drives and a removable disc capability. This enables software to be developed on one computer and easily used on another computer.

All the software developed in the Met O RRL has been written using PDP MACRO-11 assembly language, although a variety of subroutines (MACROs) have been developed to provide programmers with certain facilities usually only found in high-level languages (Davy, 1974). These facilities have aided rapid software development, and given a degree of program self-documentation. The operating system used at Malvern has been DEC DOS-11, but this has been upgraded to RT-11 Version 3.

### 3. Real-time data processing

#### 3.1 Data processing at the radar sites

Ball *et al.* (1979b) have described the software system which forms the basis of that used at the radar sites. No commercial operating system is used at the sites, the intention being to keep the on-site

computer hardware configurations as small and as simple as possible. Nevertheless, the software is still complex, involving several nested interrupt routines whose priorities are a function of time. The primary tasks carried out in real time within the main radar data-processing software modules may be summarized, in the order in which they are carried out, as follows. Some of these tasks remain to be fully implemented, and some differ from those described by Ball *et al.* (1979b).

- Control of the radar aerial elevation. Data are collected during azimuth scans at four basic elevations (nominally at 1.5°, 0.5°, 2.5° and 4°) every 5 minutes. During the lowest scan the elevation may be changed a little as a function of azimuth in order to lift the beam clear of minor obstructions.
- Input of digital amplitude data previously derived by range integration of analogue radar signals (resolution of input data is 750 m, 0.1°). Amplitude data averaged in azimuth (resolution 750 m, 1.0°).
- Correction for occultation of the beam by intervening hills and obstacles.
- Elimination of ground clutter by comparison with a stored clutter map followed by interpolation to derive weather signals in the cluttered areas.
- Correction for attenuation through rain for the 5.6 cm radars (attenuation by rain is negligible for the 10 cm radars).
- Conversion of radar reflectivity factor,  $Z$ , to rainfall rate,  $R$ , using the standard relationship  $Z = 200 R^{1.6}$ .
- Averaging in range (resolution 1.5 km, 1.0°).
- Range normalization for ranges beyond 50 km (for ranges less than 50 km the normalization is applied via hardware).
- Conversion from a large number of polar cells to a relatively small number of Cartesian cells on both 2 km and 5 km grids. (2 km grid is derived for the 0.5° elevation scan only out to a maximum range of 50 km for the Camborne, Upavon and Clee Hill radars and 75 km for the Hameldon Hill radar; 5 km grid out to 210 km maximum range at each elevation scan except 4° for which the maximum range is 105 km and 2.5° for which the maximum range is 140 km.)
- Insertion of data from higher elevation scans into badly cluttered parts of the lowest elevation data grid (referred to as the 'multi-beam' task); this is a way of overcoming the problem of extensive areas of ground clutter close to a radar site.
- Calibration of the radar using data telemetered from several rain-gauge sites at different ranges from the radar.
- Conversion to 8-bit float notation in order to reduce the amount of storage space (magnetic tape, computer core, etc.) needed for the data grids.
- Data written to 9-track, 800 bpi magnetic tape (7-inch diameter spools, 600 ft tape), the data being zero packed, and areas with no data in the corner of the Cartesian grid being removed. Data integrated over subcatchments are also written to tape. These tapes are sent to Malvern about 10 days after recording for archiving.
- Output to local users of 3-bit picture data and rainfall totals for subcatchment areas integrated over a specified period (e.g. 15 minutes, 1 hour, 24 hours).
- Output of 8-bit data to Malvern including a repeat transmission which is used in an error-checking routine at Malvern (Appendix B).

### 3.2 Data formats and types of terminal

In the Pilot Project at present data are distributed to selected Meteorological Offices and Water Authorities direct from individual radars (section 2.2). It is planned that data will also be distributed to some users after further processing (i.e. the compositing of data from individual sites, and analysis by a forecaster using the Display computer) at Malvern.



The data are formatted as a stream of 3-bit (8 intensity levels including zero) numbers framed by various sequences of control characters, and may be displayed by the user on a colour monitor using a commercially available electronic store designed by the RSRE (Ball *et al.*, 1976). This store, in the form currently available from Jasmin Electronics Ltd, may hold up to nine pictures from individual radars or four pictures of the radar composite which may be replayed in time-lapse sequence. Subcatchment data transmitted from the individual radars may be displayed on a simple strip printer (Ball *et al.*, 1979b). Data may also be recorded and replayed by users using an audio cassette recorder and a modem (modulator and demodulator unit). Figure 5 shows the maximum hardware configuration that a user could have.

### 3.3 Compositing data from several radars at Malvern

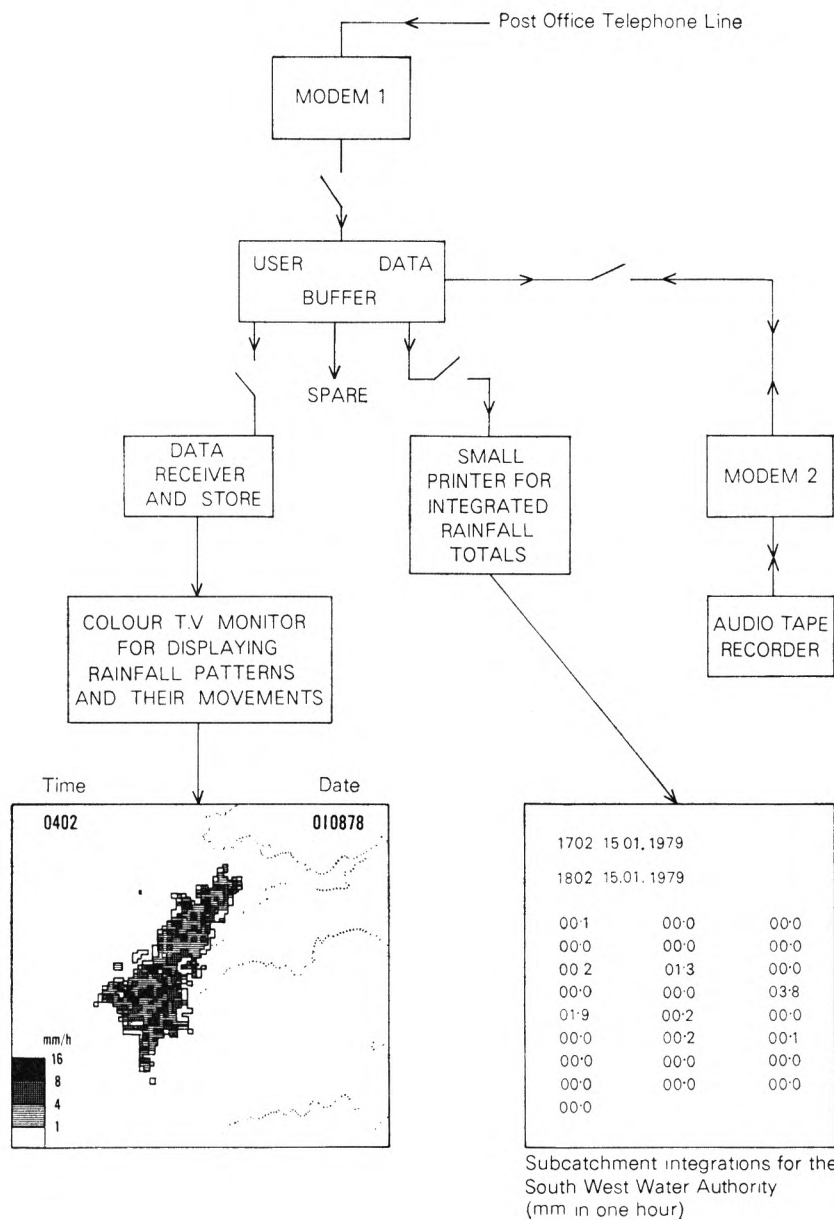
Ball *et al.* (1979b) describe software developed by the Royal Signals and Radar Establishment (RSRE) to produce, in near-real-time, a composite picture of surface rainfall based on data from three radar sites. A form of packet switching communications system was developed initially to enable control of the network as a whole to be passed from one radar site to another if necessary (Taylor, 1975). However, the need for any radar site to be able to control total network operation is not great. Moreover, it is accepted that radars which are part of separate projects (e.g. the Hameldon Hill radar) must not be interfered with by other sites. Thus, following the suggestion by Ball *et al.* (1979b) that the network communications could be simplified, we have developed new software for use in the network computer at Malvern, and in the computers at the radar sites, which uses the error-checking techniques described in Appendix B. Essentially, the network computer software adopts a passive role, 'listening' for data from the radars within a pre-set temporal window every 15 minutes.

At Malvern the data from several radar sites are processed simultaneously every 15 minutes on an interrupt basis. At present four radars are involved, but the software has been written to cope with data from at least eight radar sites. We now give a broad summary of the software which has been written to composite the data from several radars in real time; the details of the software will be described by Larke and Collier (1980). The main tasks, in the order in which they are carried out, are summarized as follows:

- Simultaneous input of data from radar sites using a 'dual buffer' technique for each site. Two buffers are allocated to each site so that data may be received in one, whilst data are being transferred to the computer disc from the other. This avoids any possible loss of data during disc transfers.
- Error-checking routines to assess whether data from the repeat transmissions are required (Appendix B).
- Empty buffers for each site alternately to the computer disc as the data are received.
- Read the data on disc (in 8-bit format) into the computer core, placing the data from each site in a composite  $128 \times 128$ , 5 km grid. A stored map gives the boundaries between radar sites.
- Write the composite data (in a  $128 \times 128$ , 5 km grid) to 9-track, 800 bpi magnetic tape (do the same for the data from each individual site on  $84 \times 84$ , 5 km grids, as back-up for the on-site tape recordings).
- Output the composite picture in 3-bit format to a local display at Malvern and eventually to other users elsewhere.

### 3.4 Satellite data processing

The need in the Pilot Project is for infra-red and visible satellite data (and combinations thereof) in digital format covering part of the British Isles and surrounding areas. Therefore the SDUS Meteosat data received at Lasham are transmitted to Met O 22 at Bracknell, where the data are digitized on a



**Figure 5.** The terminal equipment available to a user for receiving data from an individual radar via a Post Office telephone line. The user may have all the equipment shown, or a part of it (Modem 1 and the user data buffer are mandatory). Examples of the individual radar picture data and subcatchment output are also shown.

PDP11/60 computer, the area required being extracted and projected on to an extended National Grid (for compatibility with the radar data), and transmitted to Malvern, where they arrive about 5 minutes after being received at Lasham. These data can be sent, either on a  $128 \times 128$ , 10 km grid formatted in the same way as the radar data transmitted from radar sites, or on a  $256 \times 256$ , 5 km grid in a packed format. The  $128 \times 128$  grid format enables the data to be displayed on a colour monitor using an electronic store (the Jasmin store display) for radar data as described by Ball *et al.* (1979a) (see section 3.2). The  $256 \times 256$  grid format, the normal mode of transmission to Malvern, has been specified so that the data may be input to a computer-driven display system (the Display computer in Figure 3) for further processing. Data are transmitted from Darmstadt every half hour. Following an improvement in the schedule in June 1979, each picture was received at Malvern about 15 minutes after the observation time. The satellite data may be used by the Forecasting Techniques Group of the Met O RRL as soon as they are received in the Display computer (section 3.5) by using a series of simple teletype commands.

### 3.5 *The Malvern computer-driven radar-cum-satellite display*

The *total* system of processing, analysing and forecasting using radar and satellite data has been discussed by Browning (1979), and a framework in which this system could be developed has been proposed (Browning, 1980). Central to the plan is the need for a centralized computer-driven interactive display system, and such a system is being developed at Malvern (the Display computer in Figure 3).

The main tasks carried out by forecasters which require an interactive display are:

- *Meteorological analysis*: i.e. the generation of quality-controlled radar composite pictures and analysed radar-cum-satellite actuals.
- *Very-short-range forecasting of precipitation* using the radar-satellite combination as the basic data, and both objective and subjective forecasting techniques.
- *Tailoring and dissemination of actual and forecast precipitation information* using video displays, computer-to-computer links, etc. (Because the data are analysed in a digital format, dissemination via digital communications can be accomplished with a minimum delay between the production of a product and its reception by a user.)

It will take a considerable time to develop in full the required software and to optimize the way in which forecasters implement the various tasks. At present only simple software exists, which enables the radar and satellite data received at Malvern to be stored on a computer disc, combined, and displayed separately or in combination. The radar and satellite data are displayed using an electronic store developed by the RSRE, known as the Fast Update Store (Ball, 1979). Data are passed to this store very rapidly by a Direct Memory Access (DMA) transfer from the computer core, and may be displayed on either a  $256 \times 256$  grid or a  $128 \times 128$  grid. Sequences of more than 50 radar or satellite pictures (or a combination of both) may be replayed at variable speed. Forecasters may select pictures using a variety of software facilities. The details of the analysis and forecasting procedures will be the subject of future reports.

## 4. Off-line data processing

The aims of the Pilot Project include research into methods of producing short-period forecasts of precipitation, and fundamental research into the structure, mechanism and behaviour of meteorological systems. In order to achieve these aims, it is necessary to establish an extensive meteorological data base. A requirement exists (Browning, 1977) for an archive comprising radar, satellite and more conventional meteorological data. Such an archive is being established at Malvern (Table I). It will

**Table I.** *Summary of the principal data archives being established in the Pilot Project*

Data source	Archives on 9-track magnetic tape held at Malvern	Archives on media other than magnetic tape held at Malvern.
1. Data from individual radars recorded on-site—instantaneous rainfall rates.	Data from four low-elevation angles out to a range of 210 km for the two lowest elevations, for the next elevation 140 km, and for the highest elevation 105 km on a 5 km grid ( $84 \times 84$ matrix). There are 208 intensity levels. Data are composited from data at several elevations in order to produce the 'optimum' surface precipitation field. Data from this precipitation field are also available on a 2 km grid out to 50 km range for the Camborne, Upavon and Cleve Hill radars, and to 75 km range for the Hameldon Hill radar. Data from each elevation are recorded on a five-minute cycle.	
2. Data from individual radars—hourly rainfall totals.	Data from the 'optimum' surface precipitation field on both the 2 km and 5 km grids integrated over clock hours.	
3. Radar network data composited at Malvern—instantaneous rainfall rates.	Data from the 'optimum' surface precipitation field on the 5 km grid for each radar being used to form the composite. One picture from each site is received every 15 minutes. The radar composite pictures on a $128 \times 128$ grid are also recorded.	
4. Radar-compatible satellite IR and VIS data (small area).	Data are recorded on a $256 \times 256$ matrix of 5 km squares with 8 levels of intensity; data interval 30 minutes.	
5. Rain-gauge data.	Hourly totals from some 140 rain-gauges, and daily totals from about 4000 rain-gauges covering the project area (obtained from the Systems Development Branch of the Meteorological Office at Bracknell).	Facsimile displays for interesting cases only; data interval 30 minutes for Meteosat, 6 hours for TIROS-N.
6. Facsimile satellite IR and VIS data (large area).		Radiosonde data on hard copy during the passage of some precipitation systems.
7. Serial radiosonde data from Malvern.		Height-time printouts of precipitation echo intensity.
8. Vertical precipitation profiles from Malvern (not yet available).		

include rain-gauge data, facsimile satellite IR and VIS data, occasional serial radiosonde data (from a Mk 3 radiosonde system at Malvern), and data from a planned vertically pointing radar situated at Malvern. This is in addition to the instantaneous and time-integrated digital radar and satellite data.

In order to ensure that the radar data received at Malvern are of acceptable quality, a significant amount of effort has been put into the development of quality control procedures. These procedures involve the comparison, both off-line and in near-real-time, of the radar data with rain-gauge data, and the early identification of the effects of hardware faults by the Met O RRL team of forecasters. The off-line assessments of the quality of the radar data are based on hour-by-hour comparisons of the integrated radar data from each radar in the network (one picture every five minutes) with data from a network of some 40 autographic rain-gauges. The archive of rain-gauge data is held at Malvern for this specific purpose. These comparisons allow the effects of the bright band, anomalous propagation, and long-term radar calibration drift to be identified.

In order to achieve easy reference to data, one radar composite picture and one satellite picture per hour are selected from the archives for routine reproduction as hard copy (line-printer output). These hard-copy files, known as the 'condensed data set', provide the means of easily and quickly identifying cases of particular interest. An example of a printout is given in Figure 6. The condensed data set is being updated daily. On request, other data, including hourly integrations of the radar site data, may be made available as hard copy or as a magnetic-tape copy. The area integrations (subcatchments) recorded on the magnetic tapes at the radar sites are also available.

Interest in these archived data has been expressed by several Water Authorities (principally the North West Water Authority, Wessex Water Authority, Severn-Trent Water Authority and the South West Water Authority) who hope to carry out off-line case studies of hydrologically interesting events using numerical models of river catchments and empirical control rules. Arrangements have also been made to supply hourly integrations of the surface precipitation field for each radar, based on the data acquired at five-minute intervals, to the Agriculture and Hydrometeorology Branch (Met O 8) of the Meteorological Office at Bracknell. These data will be combined with available autographic and daily rain-gauge data to produce a data base which should enable practical hydrometeorological research to be carried out. This will include an assessment of the rain-gauge network densities needed in the presence of radar data to satisfy particular users of rainfall information.

## 5. Concluding remarks

The data-processing system described in this paper has been producing data only since June 1979 although data of variable quality from one or two individual radar sites have been available for much longer than this (since May 1978 in the case of the Camborne radar, and since February 1979 for the Upavon radar). The basic software package at the radar sites has now (December 1979) operated for over 18 months, although several improvements have been made to it during this period. Satellite data were received at Malvern (although not continuously recorded) from May 1978 until November 1979. The network software, and the first version of the software to combine radar and satellite data, have been extensively tested off-line, and have been in continuous use since June 1979. Over the next few years it is expected that the radar system will undergo development, such that the data it produces will steadily improve in quality. There will be a steady evolution towards greater quantitateness. Improvements will be made, for example, in the rain-gauge calibration procedures, and in correcting the data for the effects of the bright band.

More than  $5 \times 10^6$  bits of information are now being processed each hour in the total system. At present this includes data from only four radars plus digital IR and VIS images from Meteosat. However, a further radar site in the London area is already in the planning stage, and the FRONTIERS

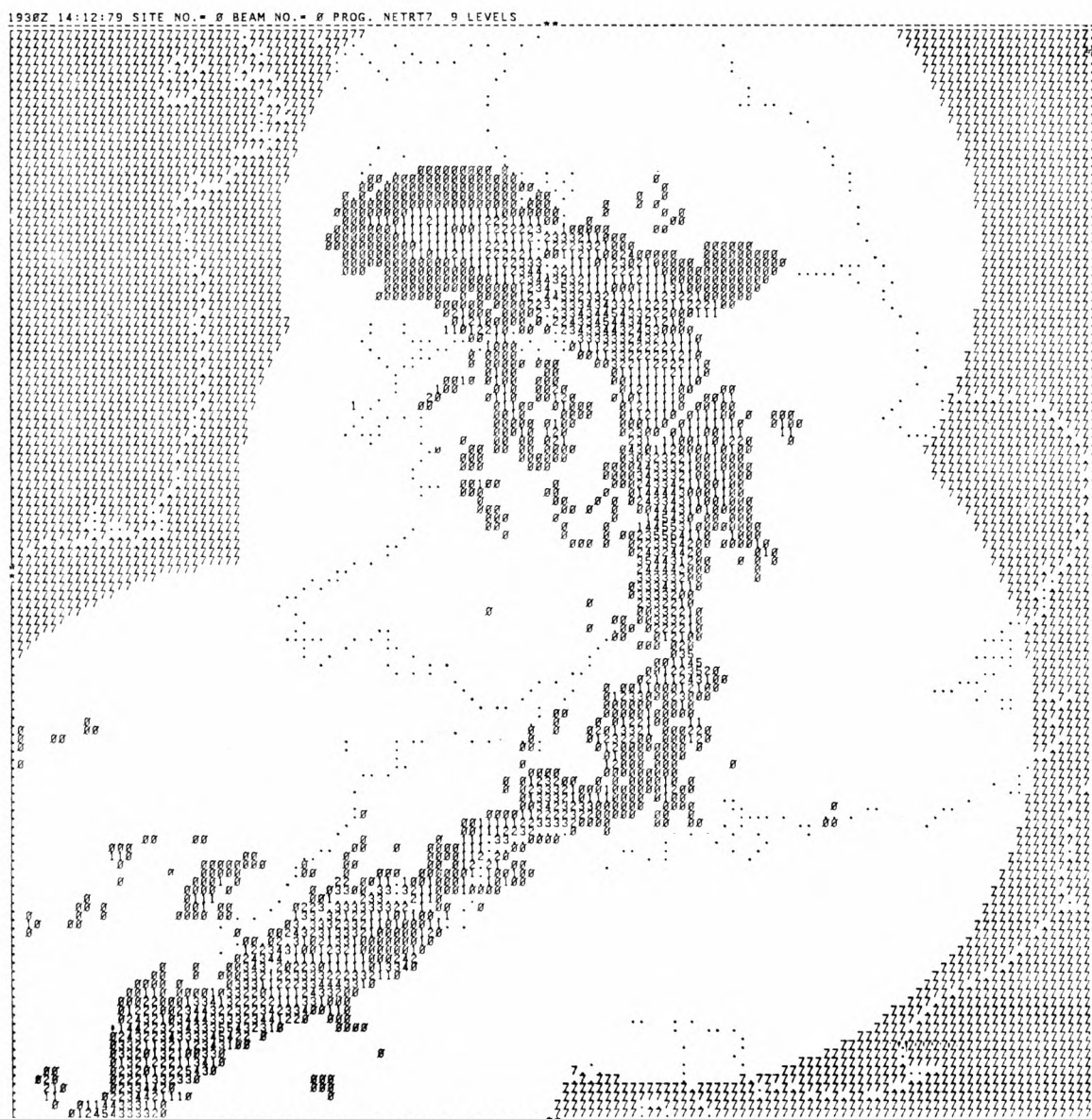


Figure 6. An example of the hard-copy product forming the 'condensed data set'. The example is a radar composite picture made up of data from the Camborne, Upavon and Clee Hill radars at 1930 GMT on 14 December 1979. Different symbols represent different rainfall rates as shown. The coastline is shown dotted. The area not observed with the radars is shown by a pattern of '7's. The key to the symbols is as follows:

Symbol	Rainfall rate ( $\text{mm h}^{-1}$ )	Symbol	Rainfall rate ( $\text{mm h}^{-1}$ )
Blank	0.0	3	4.0 to <8.0
Ø	>0 to <1.0	4	8.0 to <16.0
1	1.0 to <2.0	5	16.0 to <32.0
2	2.0 to <4.0	6	32.0 to <64.0

strategy calls for the future use of digital PDUS data from Meteosat, cloud-texture information from polar-orbiting satellites, and conventional synoptic data. Clearly the data-processing system will expand throughout the next few years of the Pilot Project.

Finally, the new generation of mesoscale numerical weather prediction (NWP) models (Carpenter *et al.*, 1978), being developed in parallel with the Pilot Project, is likely to require new sources of data for model initialization. Browning (1979) has pointed out how the FRONTIERS strategy relates to these NWP models, and has stressed the need to investigate how radar and satellite data together might be used to define the humidity field over a wide area, in a timely and more accurate fashion than that attainable by using more conventional data. The data archives being established in the Pilot Project provide the necessary data base and the opportunity to investigate how this can be achieved.

### Acknowledgements

This paper outlines work carried out by members of the Operations and Applications and Radar Techniques Groups of the Met O RRL, both headed by the author, and the RSRE Weather Radar Group led, lately, by Mr J. L. Clarke. Implementation has been undertaken by members of the Radar Facilities Group of the Met O RRL headed by Mr S. R. Smith. The Meteorological Office Short-period Weather Forecasting Pilot Project is being carried out under the leadership of Dr K. A. Browning, F.R.S., who provided considerable help in preparing this paper.

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## **Appendix A. The North West Radar Project**

The North West Radar Project is a co-operative venture supported by the Meteorological Office, the North West Water Authority, the Water Research Centre, the Central Water Planning Unit, and the Ministry of Agriculture, Fisheries and Food. The principal aims of the Project are to establish a new unmanned C-band radar on Hameldon Hill near Burnley in Lancashire (Figure 1), to integrate the radar data into the North West Water Authority's Regional Communications System (RCS), and to assess the usefulness of the radar data for operational hydrological and meteorological forecasting.

A full description of the Project aims and the data communications system has been given by Collier *et al.* (1980). Data from the radar computer at Hameldon Hill are continuously transmitted to Malvern, where they are recorded using a dedicated PDP11/34 computer which passes one picture every 15 minutes to the Network PDP11/40 computer (Figure 3). Data are also supplied to the river control centre at Catterall (Franklaw treatment works), to the NWWA Rivers Division at Warrington, and to the Meteorological Office at Manchester Airport. Communication between Hameldon Hill, Warrington and Catterall (Figure 3) is accomplished by the use of the RCS microwave communications system. The communications links to Catterall, Warrington and to Malvern are from computer to computer, but the data sent to Manchester Airport via Warrington are displayed at Manchester Airport using the Jasmin store facility (Figure 5).

## **Appendix B. Error checking**

Since the error rates on leased Post Office lines are low (usually less than one bit in  $10^6$  bits), no special error checking is carried out within the user terminal equipment used in conjunction with data transmissions from the radar sites. The data transmissions are continuously repeated for the 15-minute period between the data updates. Errors observed by the users in one transmission may be overwritten by display of the data in the next transmission. However, there is not time in the radar site software for more than two transmissions per 15 minutes of the 8-bit synchronous data to the Met O RRL at Malvern. Further transmissions within a period of 15 minutes would delay the production of the radar composite by more than 5 minutes. For these data an error-checking procedure based upon use of the automatic repeat transmission sequence has been adopted.

If an error is detected in the first transmission by the network software at Malvern which examines special block check characters generated at the radar sites, then the error may be corrected by receipt of the appropriate data blocks in the second transmission within one minute. Errors in the actual precipitation intensity values, rather than the block check characters, will not be detected until the data are displayed. No special checking code (such as the Hamming code) is used, and the procedure is a simple form of the Forward Error Correction (FEC) technique with a high degree of data redundancy, and a low (*c.* 10%) usage of the communications line. For the Hameldon Hill radar system this procedure is modified as all data processed are continually transmitted to Malvern for recording, and there is no time for repeat transmissions. In this case errors detected by examination of the block check characters are flagged but cannot be corrected.

551.501.3: 551.508.21: 681.2.08

## Radiation reference scales

By B. R. May

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### Summary

The origin of the absolute reference scales for solar radiation measurements used in the past, and the relationships between them, are described in this paper. The calibrations for the Meteorological Office National Reference pyrheliometers, which have been used to refer radiation measurements in the United Kingdom to these scales, are discussed. The implications for radiation measurements of the most recent WMO-recommended change in scale, from the International Pyrheliometric Scale (1956) to the World Radiometric Reference, is described.

### 1. History

For many purposes it is required that measured values of radiation\* are correct in the absolute sense, to be achieved by observing with instruments whose calibration can be traced ultimately to an absolute reference. In the early years of this century two main types of absolute instruments were available—the Swedish Ångström electrical compensation pyrheliometer and the Smithsonian Abbot water-flow pyrheliometer, defining the Ångström (1905) and Smithsonian (1913) radiation scales respectively. By indirect comparison it was found that these scales were not in agreement, the Smithsonian instrument registering a greater radiation than the Ångström instrument when they observed the same source; initially differences ranging from 3 to 6 per cent were observed by various workers with a mean of about 3.5 per cent. As a consequence, in 1956 an International Radiation Conference recommended that radiation measurements on a scale of greater absolute accuracy could be obtained by decreasing measurements made on the Smithsonian scale by 2 per cent and increasing those made on the Ångström scale by 1.5 per cent. This new scale was called the International Pyrheliometric Scale 1956 (IPS 1956); see, for example, Drummond (1970) and Special Committee for the International Geophysical Year (1958).

However, further measurements of the difference between the Ångström and Smithsonian scales of 4.6 per cent by Latimer (1973) and 5.0 per cent by Fröhlich (1975), in combination with the previous measurements, confirmed that the mean difference was closer to 4.5 per cent. Consequently corrected radiation measurements generated from these two original scales would still differ by about 1 per cent on average. Thus the IPS 1956 was not a well-defined scale and effectively two versions existed resulting from the use of the recommended corrections to measurements made on the Smithsonian and Ångström scales.

To ensure the comparability of radiation measurements internationally the World Meteorological Organization (WMO) recommended the establishment of a World Radiation Centre (at Davos in Switzerland) to calibrate a group of reference pyrheliometers on the basis of IPS (1956) and a system of regional and national radiation centres each with their pyrheliometers. International and Regional

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\* The word *radiation* is used for the general physical phenomenon, *irradiation* for a numerical measure of energy, and *irradiance* for a numerical measure of power, i.e. the time derivative of irradiation.

pyrheliometric comparisons (IPCs and RPCs) have been held to determine regularly the WMO-recommended calibrations for each instrument on the IPS (1956) scale. WMO appointed Kew Observatory as a Regional Radiation Centre, and hence, National Radiation Centre (NRC) for the UK. At the beginning of 1974 the NRC was transferred from Kew to Beaufort Park, the experimental site of the Meteorological Office at Bracknell.

Initially the IPS (1956) was maintained at Davos by selected Ångström pyrheliometers built to the Swedish design and there are indications from the consistency with the UK National Standard (see table on p. 18) that over the period from 1958 to 1964 the scale changed by less than 0.4 per cent. Subsequently the IPS (1956) was maintained at Davos by a mixed group of Ångström pyrheliometers of the Swedish and American (Eppley) designs, which are slightly different. This resulted in a change in the IPS (1956) which has been attributed to the influence of circumsolar radiation on measurements made with these pyrheliometers whose fields of view are different from and greater than the 0.5 degree apparent diameter of the sun. Calibrations of the Meteorological Office Ångström pyrheliometer number 583 in Stockholm and Davos indicate that irradiances measured with respect to the Swedish IPS (1956) (i.e. the Ångström scale adjusted by plus 1.5 per cent) need to be reduced by about 1.6 per cent to make them agree with those measured on the IPS (1956) as that scale was established at the 1975 IPC IV at Davos.

During the early 1960s improved absolute radiometers using the absorbing-cavity principle were being developed, and theoretical and experimental evidence indicated that their observations of radiation were more self-consistent and closer to the true value than those of previous pyrheliometers. Comparative measurements between cavity radiometers and pyrheliometers deriving their calibrations from IPS (1956) confirmed that the IPS (1956) as realized at international comparisons in 1970 and 1975 did not agree with the original definition of IPS (1956) which should have been in good agreement with the radiometer measurements; for details of these differences see Fröhlich (1975), Rodhe (1975) and Latimer (1973). After a large number of comparisons between various models of absolute cavity radiometers, recommendations were put forward that a new scale of radiation known as the World Radiometric Reference (WRR) should be brought into use. Details of the experimental basis of this scale are given by Fröhlich (1977) (Fröhlich refers to WRR as the Solar Constant Reference Scale). The WRR specified in SI units represents the absolute value of irradiance with an estimated accuracy of better than  $\pm 0.3$  per cent.

The effect of the change in scale from IPS (1956) to WRR is to increase by 2.2 per cent the values of irradiances obtained from instruments calibrated on the IPS (1956) as that scale was realized at the IPC IV at Davos in 1975. WMO recommend that WRR be brought into use from 1 January 1981, or that it be brought into use on a provisional basis before that date provided care is taken to indicate the relevant scale to which data are related.

## **2. Radiation scales and Meteorological Office practice**

Up to 1967 three old models of the Ångström pyrheliometers, numbers 24, 100A and 100B, were used as the UK reference instruments. Modern Swedish Ångström pyrheliometers numbers 583 and 587 were obtained in 1967 and 1968 respectively to replace the older instruments. The comparisons between the UK reference pyrheliometers and the results of the international comparisons show that the instruments 100B and 583 have been very stable and consequently they have been regarded as the primary reference instruments.

Up to 1957 the instrument Å100B was calibrated in Sweden and subsequently at Davos and was also submitted to two international comparisons at Davos; the instrument Å583 received an initial calibration in Sweden and thereafter took part in four international comparisons at Davos and Carpentras, in

France. The calibrations (in  $\text{W m}^{-2} (\text{amp})^{-2}$  on the IPS (1956) scale, unless stated otherwise) determined for these instruments are:

Place	Year	Instrument	
		Å100B	Å583
Sweden	1956	1015 on Å (1905) scale = 1030	
Davos	1958	1032	
Davos	1959 (IPC I)	1034	
Davos	1964 (IPC II)	1034	
Sweden	1968		5950
Carpentras	1969 (RPC II)		5820
Davos	1970 (IPC III)		5861
Davos	1975 (IPC IV)		5856 (= 5985 on WRR)
Carpentras	1978		5849

On the assumption that the IPS (1956) scale against which both instruments were calibrated in Sweden was unchanged between 1956 and 1968, the relative change of IPS (1956) as maintained at Davos over the period 1968–75 and revealed at international comparisons can be seen from these calibration figures.

All measurements made in the Meteorological Office before 1 January 1957 were related to the Ångström (1905) scale but have been increased by 1.5 per cent to make them conform to the IPS (1956) scale as it was understood at that time.

Because of the uncertainty in the constancy of the IPS (1956) and the likelihood that a new radiation scale such as the WRR would be close to the IPS (1956) as maintained in Sweden, the Meteorological Office has always used the value of  $5950 \text{ W m}^{-2} (\text{amp})^{-2}$  for Å583. The effect of this is that according to WMO recommendations following the 1975 IPC IV, all Meteorological Office-determined and archived values of irradiation are 1.6 per cent too large relative to the IPS (1956) as realized in 1975 but only 0.6 per cent too small relative to the WRR. On the WRR scale the calibration coefficient of Å583 is  $5985 \text{ W m}^{-2} (\text{amp})^{-2}$ .

In 1977 a cavity radiometer, number TMI 6764, was obtained to supplement the Office's Ångström pyrliometer. It is of a type designed by J. M. Kendall and manufactured in the USA, and indicates irradiance directly in units of  $\text{mW cm}^{-2}$ . This radiometer was submitted for the first time to an international comparison in Carpentras in 1978 at which it was established that it has a calibration factor of 0.9975, i.e. it registers only 0.25 per cent too low with reference to the WRR as determined by a direct comparison with an absolute cavity radiometer from Davos and indirectly against (with appropriate corrections) Ångström pyrliometers.

### 3. Adoption of WRR

The Meteorological Office has adopted the option given by WMO and made the change-over to WRR on 1 January 1980. From that date all calibration factors for radiation instruments are related to the WRR. All stations submitting data to the Office have been advised to adjust the already established calibration factors of their instruments to the new scale. All the instruments used by the Office and by co-operating stations will in time be re-calibrated to the new scale. Archived data up to 31 December 1979 will not be amended to WRR but all data will be annotated as to the radiation scale used. The difference of 0.6 per cent between the new scale and that previously used is considered sufficiently small compared with the accuracy of the measurements as not to warrant changing existing archived data.

If the change in the scale of reference is considered important by any user of data or instruments calibrated by the Meteorological Office, this note gives sufficient information for the necessary adjustments to be made.

### Acknowledgement

The author wishes to acknowledge the contribution made by H. E. Painter of the National Radiation Centre to the preparation of the paper.

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## Noctilucent clouds over western Europe during 1979

By D. H. McIntosh and Mary Hallissey

(Department of Meteorology, University of Edinburgh)

Table I summarizes the observations of noctilucent cloud (NLC) over western Europe during 1979 which have been reported to the Department of Meteorology, University of Edinburgh.

The times given in the second column of the Table are not necessarily the duration of the display, though appearance and disappearance times are referred to in the Notes where known. In the third column brief notes of the displays enlarge on the facts listed in other columns—NLC forms discernible, tropospheric cloud conditions, photographs and sketches available. Co-ordinates of the observing stations and selected details of elevation and azimuth appear in the remaining columns.

With the inclusion of Icelandic and Swedish information the northern hemisphere observing 'season', during which favourable viewing conditions for NLC last for a few hours centred on local midnight, is extended from about mid-April to late August. NLC occurrence outwith this season cannot be ruled out but it would be particularly helpful to have photographs referring to any such reports.

The supply of positive information during the 1979 observing season was perhaps dependent to an even greater extent than usual on the professional meteorological observer. In the 'unsocial' hours he detected the presence of the clouds during temporary, often only local, breaks in tropospheric cloud cover, on nights which the voluntary observer would have deemed unsuitable.

Forty-three sightings are listed for the 1979 season—many being no more than recognition of the presence of NLC by experienced observers, through breaks in tropospheric clouds.

On 10/11 July the most widely reported display occurred—the same date as the main display of the previous year. Photographs were taken at Milngavie at 10/15 minute intervals between 2145 and 0200 and show the development and movement of the cloud field; photographs were also taken at Edinburgh at 2315. 'Impossible' conditions for the most part prevented any sighting from Swedish stations on this night.

Routine hourly observations were made at 16 meteorological stations in the United Kingdom, 4 in Sweden, and at Reykjavik in Iceland when darkness permitted, and the sky conditions detailed in these form an important contribution to the data collection, particularly where conditions are sufficiently clear to allow a decision of 'No NLC'. 'Negative' nights are particularly significant during a possibly unbroken series of NLC appearances, or as a helpful point of reference when NLC is suspected by another observer in the vicinity. Positive reports, some accompanied by sketches, came from the 4 Swedish stations and a Swedish aircraft, 13 UK meteorological stations, 2 stations of the Irish Meteorological Service, the Peterhead Coastguard, and from voluntary observers at Douglas, Newton Stewart, Milngavie, Newton Mearns, Alrø and Fiane. A simultaneous auroral display was seen from Alrø on 26/27 July. The most southerly display was seen on 2/3 July from Bedford and Exeter.

In Edinburgh, time-lapse photography was carried out throughout the observing season; on occasions NLC was detected by eye and still photographs were taken (13/14 and 17/18 June, 7/8, 8/9 and 10/11 July). Photographs were received from Milngavie (10/11 and 14/15 July), Malin Head (4/5 July), Alrø (26/27 July) and Reykjavik (22/23 August), all of which we acknowledge with thanks.

The co-operation of the meteorological authorities of Denmark, Iceland, Ireland, Sweden and the United Kingdom, and voluntary observers in Britain, Denmark and Norway is gratefully acknowledged.

The collection, collation and publication of the written and photographic data are made possible by a grant from the Meteorological Office.

Table I. *Displays of noctilucent clouds over western Europe during 1979*

Date— night of	Times UT	Notes	Station position	Time UT	Max. elev. degrees	Limiting azimuths degrees
8/9 Apr.	0100	Bright NLC veil visible Sundsvall [? Moonlit cirrus].	62°5'N 17°5'E	0100	10	060–090
13/14 May	0200	Boulmer reported possible NLC behind tropospheric cloud.	55°5'N 01°5'W			045
15/16	2100–2340	Persistent display of very bright NLC billows in zenith, receding and fading, recorded Sundsvall.	62°5'N 17°5'E	2100 2250 2310 2340	90 70 45 40	350, 200 350, 200 340, 040 360–010
18/19	2400	NLC bands visible from Wick for the one observation. Tropospheric cloud and snow shower prevented any later recognition.	58°5'N 03°W	2400	12	
23/24	0130	Bright band of NLC distinguishable against very bright—probably NLC veil—background.	55°5'N 01°5'W	0130	3	010–060
25/26	0045, 0100	From Edinburgh report of probable NLC—low elevation; very faint glow seen Claremorris.	56°N 03°W 53°5'N 09°W	0045 0100		360 345
27/28	2300	Faint band of NLC visible Boulmer; band described as 'boomerang'-shaped.	55°5'N 01°5'W	2300	7	340
28/29	2320, 2330	NLC suspected present at 2320; revealed 2330 by breaking up of tropospheric cloud.	56°N 03°W	2330	10+	330–030
30/31	2245–0030	Medium brightness NLC bands to high elevation seen from Gotland.	57°5'N 18°5'E	2300	70	320–020
1/2 June	2230, 2300	From Edinburgh NLC suspected visible at low level behind bank of haze; seen more clearly as band of NLC from Machrihanish.	56°N 03°W 55°5'N 05°5'W 55°N 04°5'W	2230 2300 2300	5 5	340
2/3	2400–0230	Benbecula reported sky clear 2300. At 2400 bright low-level glow visible which became very bright 0100, with globular, or knotted formation noted at centre.	57°5'N 07°5'W 56°N 03°W	2400 0200 2350	5 10	310–010 360
		Faded into light sky at 0230. Report from Umeå for 2055 thought unlikely to be NLC because of sky lightness.	[64°N 20°5'E	2055	20	310–330]
9/10	2330	NLC suspected present behind bank of tropospheric cloud (approx. to 15°).	56°N 03°W 55°5'N 04°5'W	2300 2315 2345		
10/11	2315–2400	From Jönköping report of high elevation medium brightness NLC veil. No NLC visible 0015.	58°N 14°E		60 60	330–030 030–060
13/14	2350	From Edinburgh NLC suspected visible through temporary breaks in tropospheric cloud. Still photographs.	56°N 03°W			
16/17	0100, 0200	Report from Kinloss of extensive stretch of possible NLC, from 17° to 140°, bands having E–W orientation south of zenith and N–S to north of zenith. Later report was of small patches overhead.	57°5'N 03°5'W	0100 0200	140 90	
17/18	2300	From Edinburgh NLC seen extending above low bank of tropospheric cloud. Still photographs.	56°N 03°W 55°N 04°5'W	2300 2205	10+	360
18/19	2350, 0052	NLC bands and billows of medium brightness seen from Newcastle.	55°N 01°5'W	2350 0052	30 30	340–020 340–020
19/20	2215, 2400	Possible NLC visible Edinburgh, and through patches of cirrus at Machrihanish.	56°N 03°W 55°5'N 05°5'W	2215	45	045 345
22/23	2400, 0100	Extensive area of rippled formation visible Benbecula before 2400 to high elevation. Fainter, formless patches to N.	57°5'N 07°5'W	2400	50	
24/25	0100–0200	Benbecula and Dyce reported possible NLC, but description of dark lenticular patches with luminous undersides gives rise to doubt if NLC.	57°N 02°W	0100 0200 0100 0200	35 35	320–360 320–360 270 270
30 June/1 July	2400 (approx.)	Medium brightness NLC bands to high elevation seen Jönköping.	58°N 14°E	≈ 2400	90	340–010
1/2 July	0020–0103	Bright patches of NLC seen NNW and NNE from Douglas, I.O.M. NNW patch constant at 20° elevation. NNE patches spread to elevation of near 40° before fading.	54°N 04°5'W	0020 0045 0102	20 40 20	345, 045 045 345, 045
2/3	2140–2235 2335	Classic appearance—bluish-white, tenuous as seen from Exeter with interlaced wispy threads; whirl formation reported from Bedford; of shorter duration than observers considered usual for the rare appearances at these lower latitudes. Later report from Edinburgh of suspected NLC above 'red' horizon.	56°N 03°W 52°N 0°5'W 50°5'N 03°5'W	2135 2200 2145	20 12 20	330–030 340–345
3/4	2200–0010 0115	From Milngavie report of two definite but weak bands of NLC in western twilight segment. Display decreased in elevation and intensified, the bands sweeping round to give impression of large whirl. NLC also suspected later from Newton Stewart.	56°N 04°5'W 55°N 04°5'W	2206 0010 0115	40 12	315

\* to nearest 0.5 degree.

Date— night of	Times UT	Notes	Station position*	Time UT	Max. elev. <i>degrees</i>	Limiting azimuths
4/5	0130-0150	Faint veil of NLC in NNE seen Malin Head. Tropospheric cloud hampered further viewing. Photograph 0130.	55°N 07°5'W	0130	10	016-040
7/8	..2400..	NLC brightest at 2400; at 0005 tropospheric cloud patches moving to N. Still photographs.	56°N 03°W	2400	10+	330-030
8/9	2400 0015-0018	Bright NLC visible over wide azimuth through breaks in almost complete tropospheric cloud cover from both stations. Photographs—Edinburgh.	56°N 03°W 55°5'N 05°5'W	0015 2400	10 8	345-045 360
9/10	2350	Lighter sky in N denotes possible NLC. Sky generally clear apart from few patches of tropospheric cloud to N.	56°N 03°W			
10/11	2145-0200	Brightest and most widely reported display of the season; bands in zenith reported from Leuchars and traces of NLC seen in SSE segment at 0200 by observers at Kirkwall. Forms observed—veil, bands, billows herring-bone structure. Photographs from Milngavie taken to show development and movement of fine streaks near N and NE horizon. Brightness 4 recorded at Tirree at 2400 and 0100. Most southerly sighting from Ronaldsway, I.O.M. Photographs—Edinburgh and Milngavie.	59°N 03°W 58°5'N 03°W 57°5'N 02°W 57°5'N 03°5'W 57°N 02°W 56°5'N 03°W 56°5'N 07°W 56°N 03°W 56°N 04°5'W 54°N 04°5'W	2300 2400 0100 0200 2400 0100 2300 2400 0100 0100 2300 2400 0100 0200 2315 0040 0130 2250 2308 2315	70 70 135 25 30 16 17 19 16 21 60 80 25 15 16 17 13 14 11 13 15 11+	330-030 330-030 300-360 170 360 300-010 290-350 300-010 320-045 340-040 330-050 360-020 340-020 240-020 310-340 290-360 300-010 340-020 330-010 360-045 360 360 325 325 325
13/14	2200, 2300	Both stations viewed NLC in tropospheric cloud breaks; seen as very low level bright band from Aldergrove and only bright sheen from Machrihanish to 18°.	55°5'N 05°5'W 54°5'N 06°W	2300 2200 2300	18+ 6+	340-010 310-330 310-345
14/15	2045, 2400, 0100	Veil and banded structure visible (and photographed) Milngavie, through tropospheric cloud interference. Visible Håstrup and Boulmer in breaks in tropospheric cloud. Viewed also from Swedish airliner homing to Gothenburg.	57°5'N 12°E 56°N 04°5'W 55°5'N 10°E 55°5'N 01°5'W	2400 0030 2045 2400, 0100	30 7 10 6+ —	330-060 360 360 315 360
15/16	2245-2345	Faint veil of NLC and, later, bands, visible southern Sweden to moderately high elevation.	58°N 14°E 57°5'N 12°E 56°N 10°E	2245 2320 2345 2310 2130	40 40 30	340-360 330-350 340-360 360-060
19/20	2130	Brightness observed through breaks in tropospheric clouds—no further observations possible.	56°N 10°E	2130		
20/21	2055, 2230	Parallel bands of NLC visible through breaks in tropospheric clouds.	56°N 10°E	2055	7	020
21/22	0130	Bright and extensive formation of NLC—bands and cross-billows seen Alrø through large breaks in tropospheric clouds.	56°N 10°E	0130		
24/25	2200	NLC bands and cross-billows at high elevation of 50-90° seen Fiane—solar depression angle (s.d.a.) approx. 10°.	59°N 09°E	2200	90	345
26/27	2200	In Fiane NLC bands parallel W-E at 20° elevation and also in zenith. Almost clear conditions. Farther south in Alrø NLC seen low on horizon and later to increase in elevation and brightness until obscured by dawn light. The display appeared simultaneously with aurora. (Photographs—Alrø.)	59°N 09°E 56°N 10°E	2200 2200 2240 2400 0040	20 90 3 6 10	360 060
31 July/1 Aug. 6/7 Aug.	2400 0300	Faint veil of NLC seen Aldergrove. Single patch of NLC to NNE seen Kinloss. Sky conditions 2200-0200 would have allowed presence of NLC to be identified.	54°5'N 06°W 57°5'N 03°5'W	2400 0300	10 22	360-030 040-045
10/11	2220, 2250	Bands of NLC, faint to medium brightness visible Sundsvall.	62°5'N 17°5'E	2220 2250	35 30	300-360 340-030
11/12	2200, 2300	Wisps of NLC, E of N, seen Kirkwall, disappearing slowly at time of later observation.	59°N 03°W	2200		010
22/23	2230-2330	Photographs taken Reykjavik thought not to be NLC, but suspected sighting noted.	65°N 22°W			
24/25	2115-0115	Very bright NLC visible Sundsvall, for much of time s.d.a. 10-16°. In zenith and to south of zenith for much of display. Veil, band and billow formation; obscured by tropospheric cloud 0115.	62°5'N 17°5'E	2115 2215 2315 0015 0045	150 160 150 150 30	170-210 170-280 150-270 180-240 040-090



## Reviews

*Solar-terrestrial influences on weather and climate*, edited by B. M. McCormac and T. A. Seliga. 240 mm × 160 mm, pp. xiii + 346, *illus.* D. Reidel Publishing Company, Dordrecht: Holland/Boston: USA/London: England, 1979. Price Dfl 45, US \$24, £21.

This book contains the proceedings of a symposium/workshop held in Ohio State University in August 1978, and documents the progress of the research into the effect of fluctuations in the sun on weather and climate. The research has two parts: correlating atmospheric effects with solar events, and establishing viable physical connections between them. The most convincing paper under the first heading is the presentation of evidence by Mitchell *et al.* for a 22-year cycle of precipitation in the western USA (page 125). However, some of the other papers on solar-terrestrial correlations are less well founded, and until physical connections are established (they are only suggested in this book), the work in this field will remain highly speculative. But the book contains a useful cross-section of current research and some discussion of statistical techniques.

D. E. Parker

*Concepts in climatology*, by P. R. Crowe. 235 mm × 150 mm, pp. xx + 589, *illus.* Longman Group Ltd, Harlow, 1979. Price £6.50 (paperback student edition).

This is a reprint of a book published in 1971. The only amendments are a satellite photograph on the cover, and a list of errata and afterthoughts inside the cover. Therefore the descriptive sections do not have advantage of the most recent data, and the chapter on climatic change is completely outdated, having no mention of the numerical models and palaeoclimatic investigations of the last ten years.

D. E. Parker

*Ice ages: solving the mystery*, by John Imbrie and Katherine Palmer Imbrie. 240 mm × 155 mm, pp. 224, *illus.* Macmillan Press, London, 1979. Price £6.95.

This history of research into the ice ages is written in a non-technical style well suited to the general reader. The record begins with a fascinating account of how leading geologists were convinced by the evidence of scratched rocks and erratic boulders that an ice age had occurred. The subsequent discovery of multiple layers of glacial deposits led to the need for a theory of the succession of ice ages, and this book concentrates on what is now known as the Milankovitch theory, that regular changes in the earth's orbit are responsible.

Chapters 4 to 7 deal with the early development by Adhemar and Croll of the astronomical theory of the ice ages. The many personal anecdotes and character sketches enliven the account—and perhaps boost the popular image of the ivory-tower scientist. Thus James Croll could write 'The strong natural tendency of my mind towards abstract thinking somehow unsuited me for the practical details of daily work', and Milankovitch came 'under the spell of infinity'—with the help of a few bottles of wine! Unfortunately in some places the graphic language could mislead. For example 'the wind from the north whipped their faces' (referring to Stone Age hunters) could give the impression that northerly winds were predominant during the ice age, whereas in fact a global cooling caused by changes in the radiation supply would not have required northerly winds: the southerly winds would have been colder than nowadays too.

Milankovitch's aim was to calculate in detail the past and present climates using orbital data to determine the global distribution of incoming solar radiation. The account of his life's work is attractively presented in the book and shows his determination to succeed, even in prison. The authors are

clearly strong advocates of the Milankovitch theory, but before considering it they give a good brief summary of some other theories. However, it would have been fair also to have pointed out the imperfections in the fit of the Milankovitch theory: for example in Figures 38 and 40 the intervals between peaks of warmth are 120 000 years for the most recent cycle but only about 75 000 years in the previous five cycles. The problem of which season's radiation is critical for ice-sheets is given due attention and is left fairly open until near the end of the book when the results of spectral analysis of deep-sea cores are taken to indicate the phase relationships between orbital changes and ice ages. Unfortunately this knowledge does not clarify what mechanisms are involved, so the forecast of the future, generally cautious and balanced, is, for the long term, an extrapolation of periodicities. If the climatic reaction to orbital changes had been perfectly regular this would have been acceptable, but given the irregularities in the past, the forecast should have been worded accordingly.

The first author (J. Imbrie) is one of the leading workers with isotopic variations in deep-sea cores, and the accounts of this work are very clear and intriguing. However, there are a few errors in some of the astronomical sections. For example (see page 71) the excess of daylight over darkness in the northern hemisphere is 168 hours at the North Pole only, and decreases equatorwards. Also the argument on page 75 is suspect: in a given hemisphere the total solar heating per year will be greatest when the earth is nearest the sun in summer, because the variations of  $(1 \div (\text{distance to sun})^2)$  will have more effect on the larger quantity involved in summer.

The earlier chapters make mention of the conflict of religious belief and scientific theory. The authors would have done well to state that the conflict is one of basic principles, not of conflicting observations. The basis of all scientific research into the distant past is the principle of 'uniformitarianism', i.e. that the laws of nature have always been the same as they are now. Research could not proceed without such an assumption, and the results should be taken as true in so far as that assumption holds. Belief in God's creative and other activities in the past is not intellectual suicide but the choice of a different set of basic principles.

In summary, the authors have produced an interesting account for the general reader, but it should be read with more discriminating thought and care than the general reader may be likely to take.

D. E. Parker

## Notes and news

### **International Conference on Climate and Offshore Energy Resources, 21–23 October 1980, London—Preliminary Announcement**

This conference will be organized jointly by the Society for Underwater Technology, the Royal Meteorological Society, and the American Meteorological Society, and will be held at the Royal Society, 6 Carlton House Terrace, London SW1.

**Objectives.** To review and discuss, at an interdisciplinary level, recent developments in meteorology and oceanography relevant to the harnessing of offshore energy resources.

To consider, on the basis of current knowledge and understanding of climate, the likely implications for total energy demand and offshore production into the next century.

**Opening address** by Roy Jenkins, President, Commission of the European Communities.

**Conference Dinner.** The Conference Dinner will be held on the evening of 22 October 1980 at the Guildhall, City of London. Attendance at the dinner will be available to Conference participants on payment of a charge in addition to the Conference registration fee.

**Registration.** Attendance at the Conference will be limited to those numbers that can be effectively accommodated by the Royal Society's meeting and catering facilities.

Registration charges are £120.00 per delegate plus £15.00 VAT, and cover:

- (1) attendance throughout the Conference;
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- (5) proceedings at an 'at cost' charge.

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#### *Climate, energy and man*

Climate and world energy demand (including variations in both space and time).

Lessons from the past (including reference to history of climate, limited resources, environmental pressures, etc.).

Man's impact on climate.

#### *Studying the atmosphere and oceans*

The atmosphere—its climate and what influences it.

Oceans and ocean currents—their influence on climate.

Requirements of industry offshore.

#### *Techniques of prediction*

Principles of climate modelling (including statistical methods).

Applications of climate modelling.

Prediction using general circulation models.

#### *Offshore energy resources*

Exploitation of potential oil and gas resources offshore.

Ocean energy—waves.

Ocean energy—tides and currents.

Offshore thermal energy conversion.

#### *The next century*

Which way climate?

The World Climate Program.

How much offshore energy?

Enquiries concerning the Conference should be directed to:

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Bracknell,  
Berks., RG12 1BX.

### **Professor Gordon Manley, M.A., D.Sc.**

We regret to record the death at Cambridge on 29 January 1980 of Gordon Manley, M.A., D.Sc., Emeritus Professor of Environmental Studies in the University of Lancaster; he was the doyen of British climatologists and in the course of a long and distinguished career received many honours from his fellow meteorologists.

Professor Manley was educated at Queen Elizabeth's School, Blackburn, the University of Manchester, and Gonville and Caius College, Cambridge. His first appointment, in 1925, was to the Meteorological Office, when he became a Junior Professional Assistant at Kew Observatory. After taking part in the North Greenland Expedition of 1926, he resigned from the Office to enter academic life, holding lectureships at the Universities of Birmingham, Durham and Cambridge before becoming Professor of Geography at Bedford College, University of London, in 1948.

He left Bedford College in 1964, when he was already 62 years old, to become the first Professor of Environmental Studies in the new University of Lancaster, retiring from this appointment in 1968 although he continued to maintain a close connection with the University and the work that went on there.

He was awarded the Buchan Prize of the Royal Meteorological Society in 1963 and was Symons Lecturer in 1944. He served as President of the Society from 1945 to 1946 and in 1976 he was elected an Honorary Fellow. From 1955 to 1961 he was Correspondent for Glaciology on the British National Committee for the International Geophysical Year. From 1958 to 1962 he served on the Synoptic, Dynamical and Climatological Subcommittee of the Meteorological Research Committee.

Gordon Manley continued to work until the very end of his life, and indeed a paper on the climatology of the northern Pennines is now being edited for the *Meteorological Magazine* thereby adding to the long list of his publications on weather and climate, in particular those of northern upland Britain. His earlier researches included studies of the helm wind phenomenon of the Cross Fell range in Cumbria and of polar climatology, and while he was at Durham he set up and ran an observing station at Moorhouse in upper Teesdale, at a height of 1825 feet; he played a large part in establishing and directing the activities of the Snow Survey, himself preparing reports for the first two seasons of 1938/39 and 1939/40. In later years he concentrated on historical studies of the British climate, subjecting many long instrumental records to critical examination and adjustment in the light of all the relevant evidence; one outstanding result of this work was his series 'Central England temperatures: monthly means 1959 to 1973' published in final form in 1974 which provides a fundamental source of information for researchers into climatic change. His delightful book, 'Climate and the British Scene', helped to make him known to a wider public than that of professional meteorologists, as did his frequent articles in the *Guardian*.

He was an affable and courteous man, endlessly interested in all manifestations of weather, climate, and climatic change whether real or imagined. He belonged to the generation before that of computers and climate modelling and, perhaps in consequence, would frequently warn his younger listeners that they must maintain contact with reality by thoroughly knowing and understanding their primary sources of data, i.e. the original observations—how reliable they were and how they could be affected by exposure or by human and instrumental errors.



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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

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Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd, 24-28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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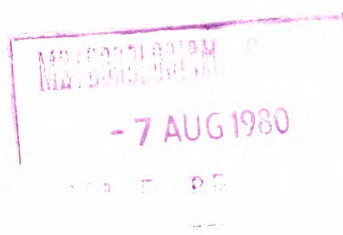
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 6/80

Annual subscription £21.18 including postage  
ISBN 0 11 722062 0  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

July 1980

Met.O. 931 No. 1296 Vol. 109

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# THE METEOROLOGICAL MAGAZINE

No. 1296, July 1980, Vol. 109

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## Urban effects on trends of annual and seasonal rainfall in the London area

By R. C. Tabony

(Meteorological Office, Bracknell)

### Summary

The large amount of monthly rainfall data available for the period from 1911 to 1970 was used to see if any effects of urbanization are present in annual and summer half-year totals of rainfall in the London area. Difficulties of interpretation due to changes of site and instrumentation caused trends obtained from rain-gauge records to vary by several per cent per decade between points only a few kilometres apart, so comparisons were made with data extracted from maps of percentage of average rainfall in which these errors had been removed. These showed that changes of site and instrumentation accounted for approximately half the variance of trends obtained from rain-gauge data and when these were taken into account the patterns of rainfall trends over London did not display any features which could be attributed to urbanization.

### 1. Introduction

The possible effects of urban areas on precipitation have been attracting increasing attention recently, particularly in regard to the incidence of short-duration heavy rainstorms of convective origin. It has been suggested that the frequency and severity of convective storms over metropolitan areas combine to give a significant increase in annual rainfall. Such effects, if present, should be taken into account in the production of long, homogeneous rainfall series intended for the study of climatic change, many of which rely heavily on data from urban areas, for example the series for Manchester compiled by Manley (1973, 1976).

Compared with their surroundings, built-up areas are associated with:

- (i) Higher values of temperature and absolute humidity.
- (ii) Increased concentrations of condensation and freezing nuclei.
- (iii) Increased mechanical turbulence.

Thus, it is reasonable to expect that urbanization will increase the frequency and severity of convective rainstorms. A good review of urbanization effects on precipitation in general is provided by Landsberg (1974). Most of the recent work has been done in the USA, in particular project METROMEX, based

on the city of St Louis. In an initial study, Huff and Changnon (1972) claimed increases of 6–15 per cent in the summer rainfall totals based on observations from 50 rain-gauges over an area of 28 000 km<sup>2</sup> from 1941 to 1968. In the latest report from project METROMEX, Changnon *et al.* (1977) used radar and dense networks of gauges to identify areas of high rainfall near an industrial complex during the period 1971–75. More recently, however, Pittock (1977) has shown that changes in rainfall over Washington State which had been ascribed to man's activities can be related to changes in the latitude of the subtropical anticyclone. For the London area, Atkinson has made a number of case studies of convective storms, an investigation into the famous Hampstead storm being the latest (Atkinson, 1977).

In this study use is made of the large amount of monthly rainfall data available in the London area for the period 1911–70. Linear trends of seasonal and annual rainfall are mapped to see if any patterns are present which may be attributable to urbanization. It is appreciated that any climatic change will not necessarily be linear, but over a period of 60 years, this is a convenient approximation to make. In order to interpret the patterns, a statistical background of the kind of variations which could occur by chance is required. The standard error of a trend associated with a perfect rain-gauge can be calculated from a knowledge of the variability of rainfall, but changes in site and instrumentation will make the errors associated with real observations larger. The differences are estimated by comparing trends calculated from rain-gauge observations with trends calculated from data derived from maps of percentage of average rainfall. In the latter data set, errors due to changes of site and instrumentation will have been largely eliminated.

## 2. Data

Monthly rainfall totals from practically all UK rainfall stations with complete records in the periods 1911–70 and 1941–70 are available in machinable form within the Meteorological Office. The number of stations available is about 700 for the period 1911–70, and 2000 for 1941–70, but they are not uniformly distributed. Their distribution for 1911–70 is shown in Figure 1. Locally dense networks existed in the London area and parts of north-west England which did not allow all the points to be plotted automatically on a 1:1.5 million scale map. Figure 1 illustrates a network of about 500 stations which could be plotted on such a map. For an area of 12 000 km<sup>2</sup> centred on London, 75 stations were available from 1911 to 1970, while during the period from 1941 to 1970 the number rises to 240.

Maps of annual rainfall over the British Isles, expressed as a percentage of average, have been published for the years from 1868 to 1923 by the Royal Meteorological Society (1926) and for subsequent years by the Meteorological Office in *British Rainfall*. With the exception of the year 1941, Ireland was included in the maps published in *British Rainfall* up to 1945. From 1948 onwards, the data for the Republic of Ireland are in the *Irish Monthly Weather Report*; for 1941, 1946 and 1947 the data were kindly supplied by the Irish Meteorological Service. For the United Kingdom the averages used were for 1881–1915 up to 1957 and 1916–50 thereafter. For Ireland, the averages used were for 1881–1915 up to 1955, for 1916–50 from 1956 to 1965, and for 1931–60 thereafter. Annual values were extracted from the maps for 59 points formed by the intersection of lines of latitude and longitude over the British Isles. These were then converted to a percentage of average for the period 1871–1970, and are referred to as the 'map' data.

As part of another piece of work, as yet unpublished, the writer collected series of monthly rainfalls representative of 185 sites in Europe for the period from 1861 to 1970. Comparisons with neighbouring gauges were used to construct long series from which all major inhomogeneities were removed. Records for 48 of the sites which lie within the British Isles are referred to as the 'homogeneous' data.

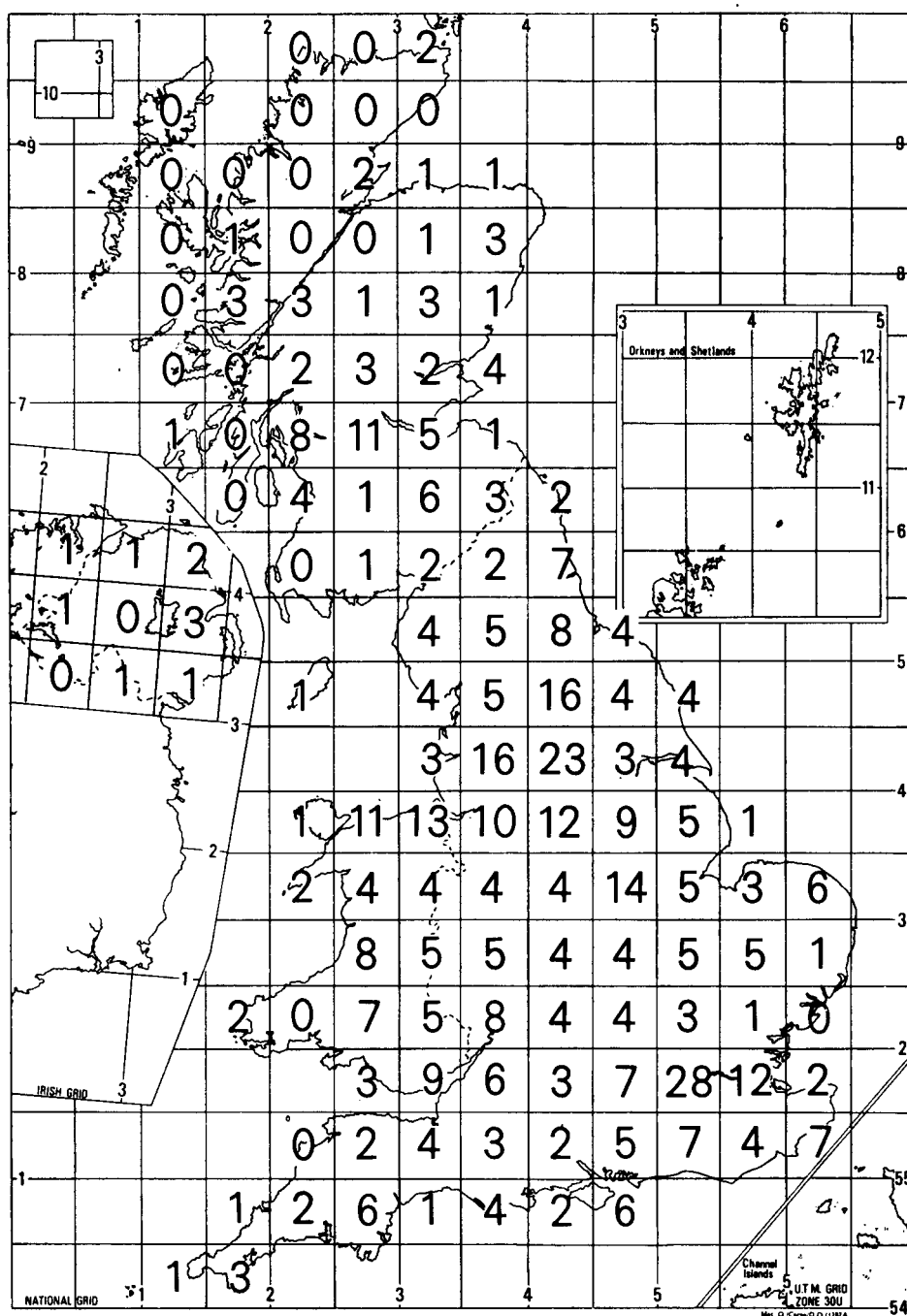


Figure 1. Network of rainfall stations with complete records from 1911 to 1970. The figures represent the number of stations in each square.

### 3. Spatial coherence of rainfall and rainfall trends

Rainfall is notoriously variable in both space and time. The high spatial variability is illustrated in Figure 2, which shows the rainfall over the United Kingdom for the year 1973 expressed as a percentage of average. The map is based on observations from about 2500 stations, and isopleths are drawn at intervals of 17 per cent, which corresponds approximately to one standard deviation. Points only a few tens of kilometres apart can be seen to have rainfall amounts which differ from one another by up to three standard deviations. When maps are drawn for rainfall summed over different durations and isopleths are drawn at intervals of one standard deviation, the spatial coherence is seen to vary little for time-scales ranging from a month to a decade.

Figure 3 displays the trend of annual rainfall from 1911 to 1970, as calculated by the method of least squares, from a dense network of stations in north-west England. Values are seen to range from  $-4.0$  to  $+5.9$  per cent per decade, with very little spatial coherence. The observed trends are due to genuine fluctuations in rainfall plus errors caused by changes of site and instrumentation. In order to aid interpretation of Figure 3, estimates of the standard errors of rainfall trends obtained from a perfect site are made below.

### 4. Standard errors of rainfall trends

In a linear regression of the form

$$y = ax + b$$

the standard error of the gradient  $a$  is given by Snedecor and Cochran (1967) (see pp. 138, 139) as

$$[SE(a)]^2 = \frac{\Sigma Y^2 - [(\Sigma XY)^2 / \Sigma X^2]}{(N - 2)\Sigma X^2}, \quad \dots \dots \dots (1)$$

where  $X$  and  $Y$  represent deviations from the mean values of  $x$  and  $y$  respectively, and  $N$  is the number of pairs of observations of  $x$  and  $y$ . For the case under discussion, the variable  $y$  may be equated with rainfall expressed as a fraction of average and  $x$  as time in years;  $x$  will therefore run from 1 to  $N$ , and it may be shown that

$$\Sigma X^2 = N(N^2 - 1)/12.$$

The covariance between rainfall and time will be very small, so that the second term in the numerator of equation (1) may be neglected. Assuming  $N$  to be large, equation (1) reduces to

$$[SE(a)]^2 = 12 \Sigma Y^2 / N^4. \quad \dots \dots \dots (2)$$

$\Sigma Y^2 / N$  may be equated with the variance of rainfall expressed as a fraction of average, so we have

$$SE(a) = (12)^{0.5} C_v / N^{1.5}, \quad \dots \dots \dots (3)$$

where  $C_v$  is the coefficient of variation of rainfall. Insertion into equation (3) of  $C_v = 0.16$  and  $N = 60$ , figures appropriate to the rainfall data in Figure 3, yields an estimate of the standard error of the trend of 1.2 per cent per decade. Thus the observed trends over north-west England extend through a range of eight standard deviations if it is assumed that there are no errors due to changes of site and instrumentation.

### 5. A comparison between the 'map' and 'homogeneous' data

These two data sets are the results of efforts to eliminate errors due to changes in site and instrumentation. In the case of the map data, the mapping procedure will have smoothed out any site and instrumental errors, but it may also have smoothed out some genuine rainfall variations. In the case of the

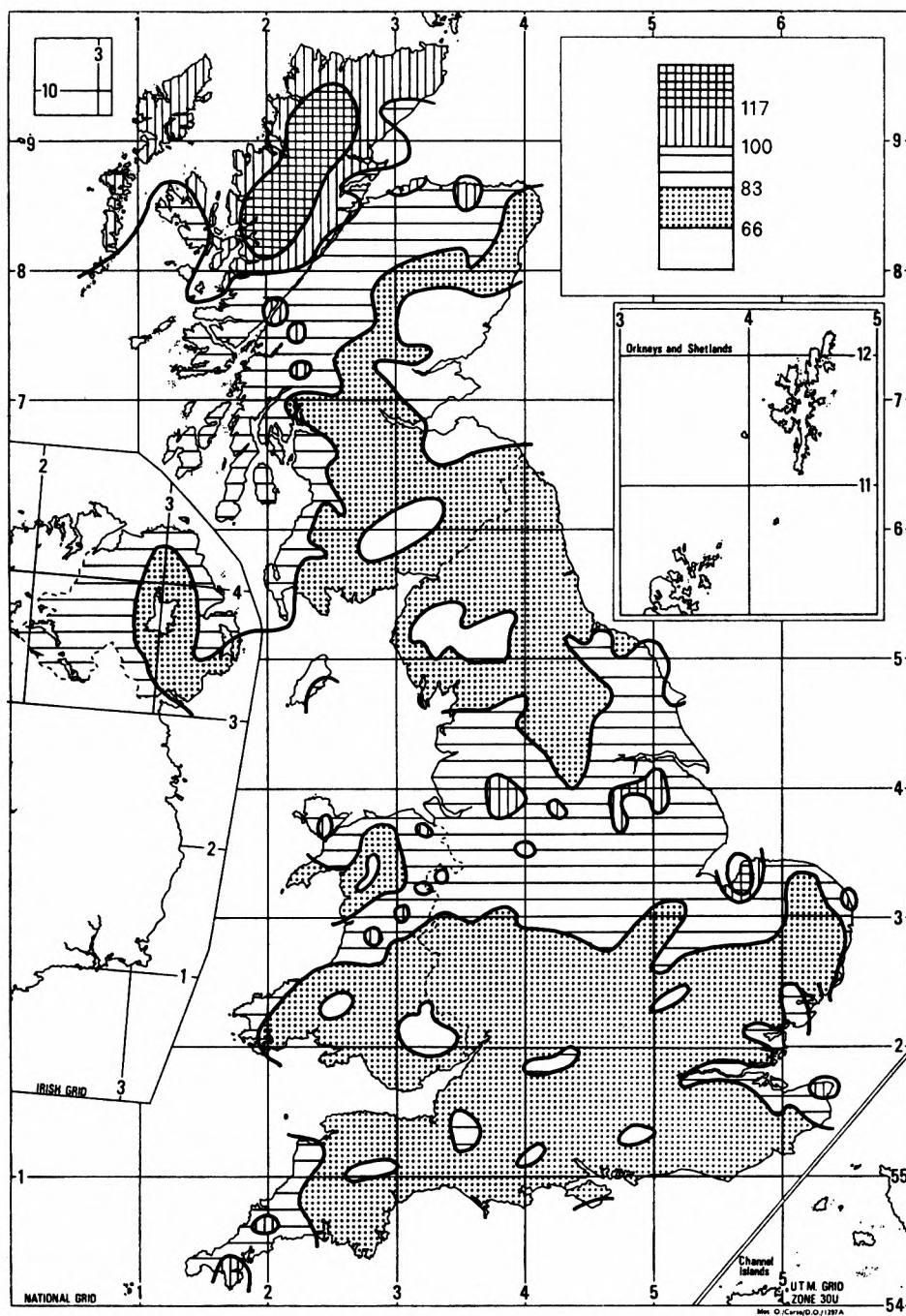


Figure 2. Rainfall for the year 1973 expressed as a percentage of the annual average for 1916-50.

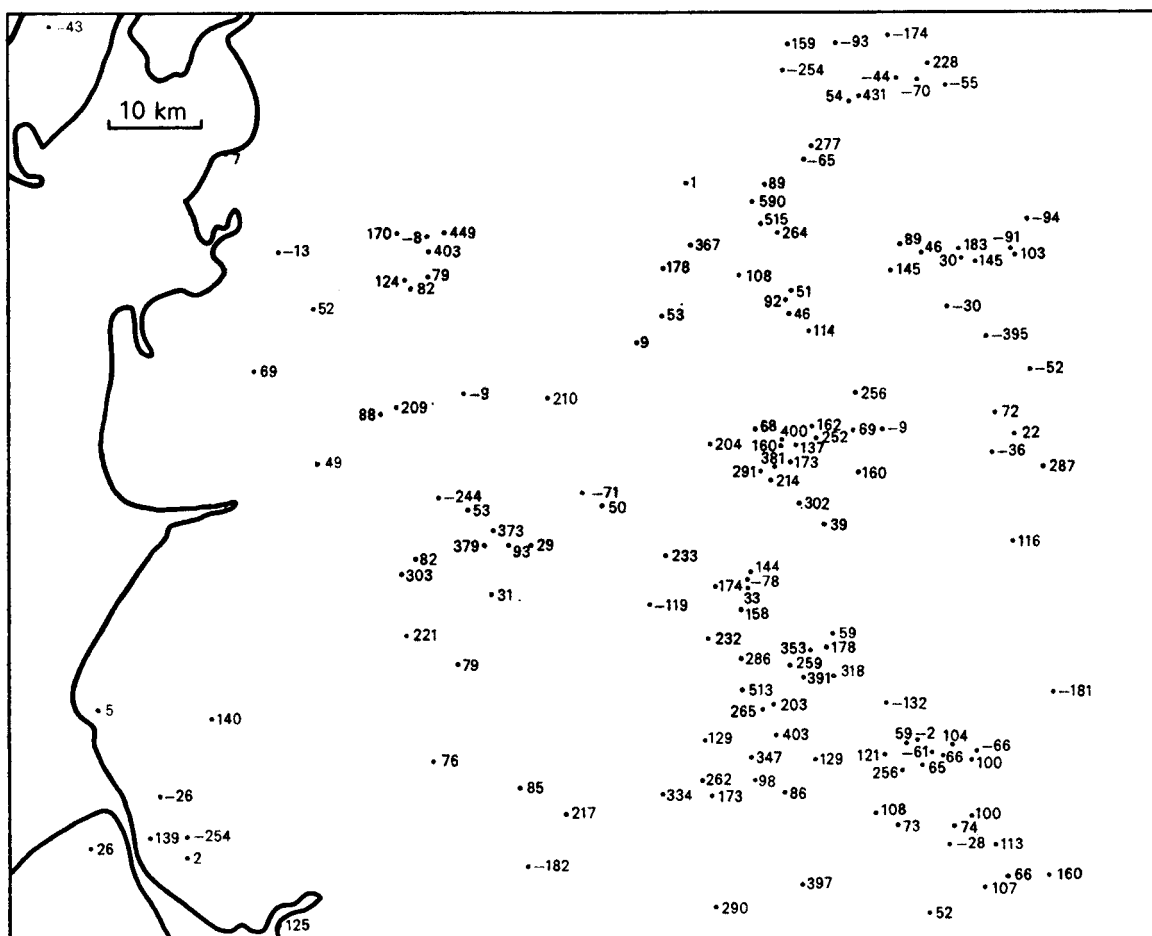


Figure 3. Trend of annual rainfall over north-west England from 1911 to 1970 (per cent per decade  $\times 100$ ).

homogeneous data, no rainfall fluctuations will have been reduced in amplitude, but not all the site and instrumental errors may have been detected. The different methods of construction of the two data sets can be illustrated by calculating the variance of annual rainfall. In the map data, excessive smoothing will result in an underestimate of the true variance, while in the homogeneous data, any uncorrected errors will tend to yield an overestimate of the true variance.

The coefficient of variation of annual rainfall over the British Isles for the period 1871–1970 as obtained from both data sets is shown in Figure 4. Values for the map data are slightly less than those from the stations with homogeneous records, but the closeness of the results indicates that both data sets are of a high standard.

Trends of annual rainfall from the map and homogeneous data for the period 1871–1970 are presented in Figure 5. Both data sets reveal similar patterns, with a belt of negative values stretching from south-west to north-east and separating positive areas to the north-west and south-east. The strong spatial

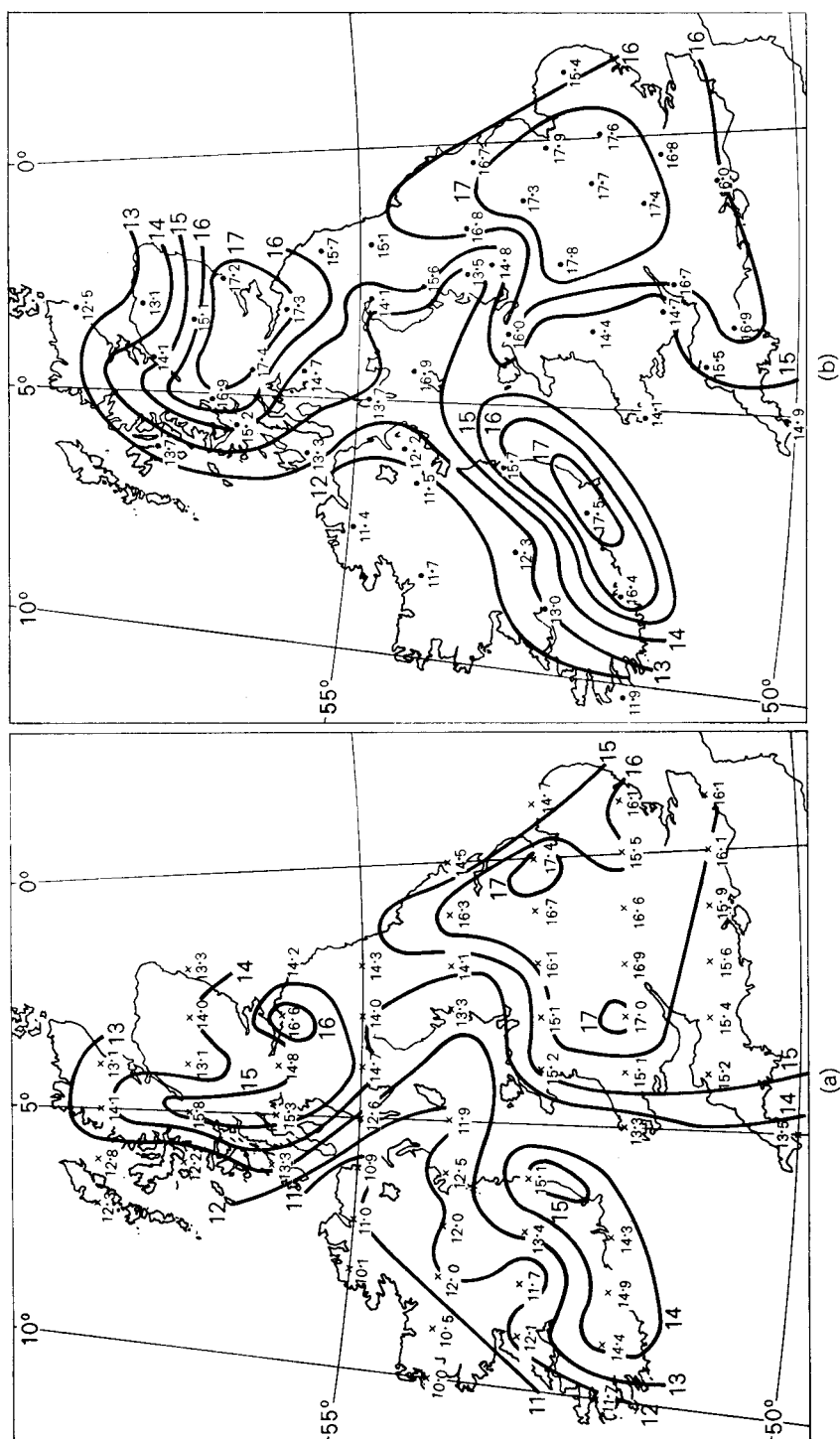


Figure 4. Coefficient of variation of annual rainfall 1871-1970 (per cent)—(a) derived from map data, (b) derived from stations with homogeneous records. Standard error c. 1.2 per cent.

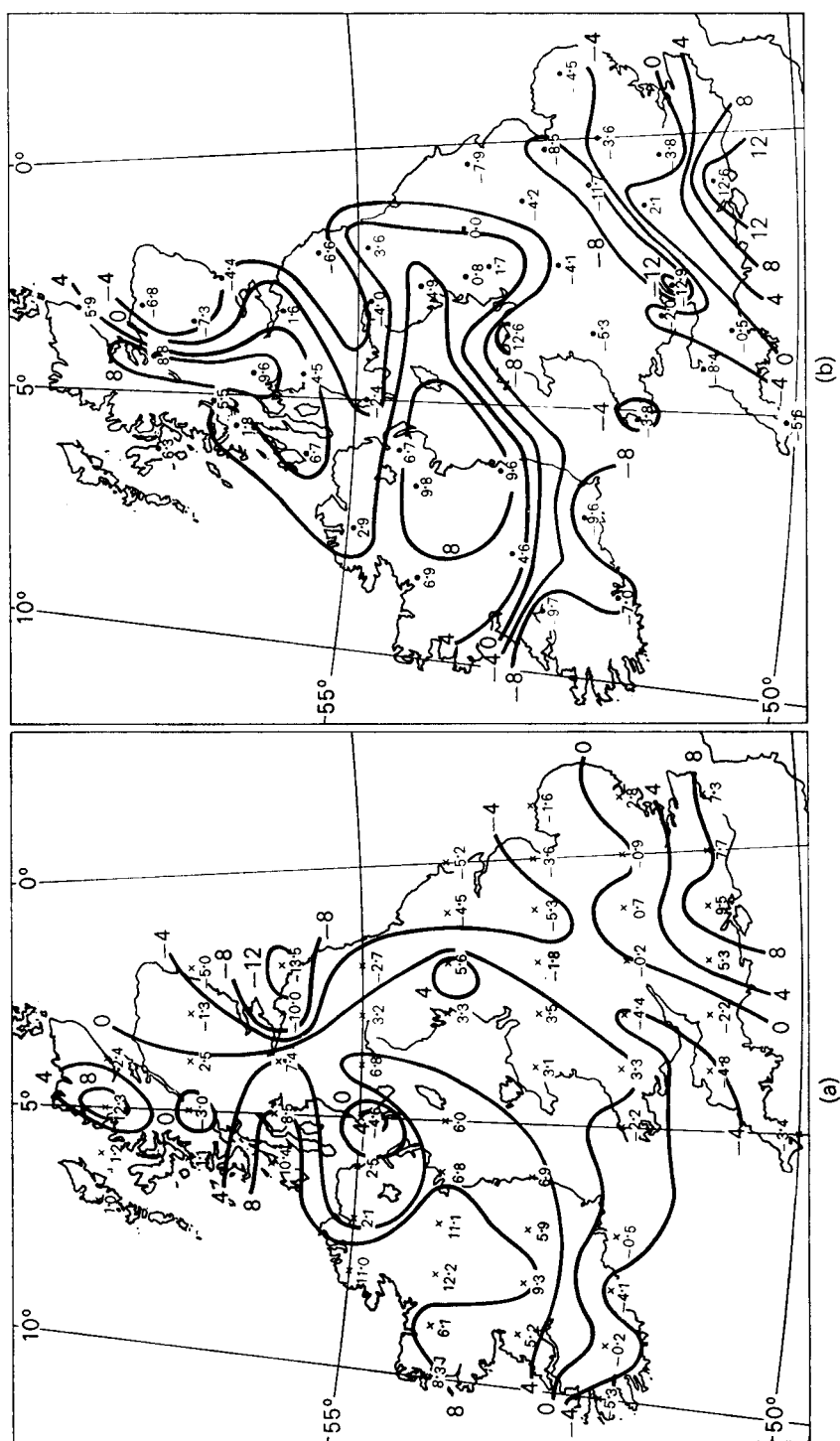


Figure 5. Trends of annual rainfall (per cent per century) from 1871 to 1970—(a) derived from map data, (b) derived from stations with homogeneous records. Standard error c. 5.9 per cent per century.



coherence forms a marked contrast to that associated with the 'raw' rain-gauge data presented in Figure 3. The coherence of the homogeneous data is, however, less than that of the map data, probably owing to the great difficulties involved in eliminating all the inhomogeneities from raw rain-gauge data. For this reason, the map data are considered to be slightly superior to the homogeneous data.

# 6. Effect of site and instrumental changes on trends derived from rain-gauges

If perfect rainfall records were available for every rain-gauge location, then two trends could be calculated, one from the gauge observations and the other from the perfect record. From  $N$  rain-gauges in a given area, one could calculate the variance of the trends from the gauge observations and from the perfect records. The variance of the gauge observations would be the larger, owing to the effects of changes in site and instrumentation. The ratio of the two figures would then give the proportion of the variance of observed trends due to genuine changes in rainfall trends across the area.

The map data were taken as an approximation to a perfect rainfall time series. Trends for the period from 1911 to 1970 were computed for 26 points in England and Wales, and their standard deviation about an assumed population mean of zero calculated. This was compared with the standard deviation of trends obtained from a similar number (25) of uniformly distributed but otherwise randomly selected rain-gauge records in England and Wales. Thus the comparison is not between the variances of trends from 'perfect' and gauge observations for exactly the same locations, but for different but uniformly distributed points in England and Wales.

The ratio of the two standard deviations may depend on the number of points and the number of years used. The number of points derived from maps was always 26, but the number of rain-gauges was varied from 25 to 200, while the length of epoch over which the trends were calculated ranged from 15 to 60 years. The results are presented in Table I, and show that the standard deviation derived from the

**Table I.** *Standard deviation of rainfall trends over England and Wales (per cent per decade) as obtained from various data sets*

Data set	Epoch						
	1911-70	1911-40	1941-70	1911-25	1926-40	1941-55	1956-70
26 points from maps	0.63	1.88	1.93	5.26	7.28	3.75	4.28
25 gauges	1.46	3.12	2.16	8.46	7.68	6.79	5.96
50 gauges	1.49	3.26	2.45	7.74	7.60	5.74	5.79
100 gauges	1.41	3.24	2.46	7.71	7.55	6.04	6.11
200 gauges	1.59	3.38	2.37	7.69	7.99	5.79	5.87
26 points from maps							
Average for gauges	0.42*	0.58	0.82	0.67	0.95	0.62	0.72
		0.70**				0.74†	

\* 60-year epoch \*\* mean of two 30-year epochs † mean of four 15-year epochs

gauges varies only slightly with the number used. For this reason the mean of the four sets of figures obtained for the gauges was used in making comparisons with the results from the 'map' data. The absolute values of the standard deviations increase as the length of epoch decreases, but the ratio of the standard deviations of map data to gauge observations ranges from 0.42 for 1911-70 to 0.70 for 30-year epochs and 0.74 for 15-year epochs. There is therefore some suggestion that the proportion of variance accounted for by genuine rainfall fluctuations decreases as the length of epoch increases. As a broad generalization however, it may be said that fluctuations of rainfall and errors due to site and instrumental changes contribute about equally (50 per cent  $\approx$  0.7<sup>2</sup>) to the variance of trends obtained from rain-gauge records. For the epoch 1911-70, the ratio of 0.42 indicates that the standard errors calculated

in section 4, which apply only to genuine fluctuations of rainfall, should be more than doubled (multiplied by 1/0.42) to take account of site and instrumental changes. From this it is inferred that the trends displayed in Figure 3 indicate that genuine rainfall trends within the area covered ranged through 4 (as opposed to 8) standard deviations.

### 7. Annual and seasonal rainfall in the London area from 1911 to 1970

Trends of annual rainfall in the London area during the period from 1911 to 1970 are shown in Figure 6. It reveals small decreases except for an area in north London and to the east of the conurbation. The pattern bears no obvious relationship to the built-up area, and is not significant when the standard error of 1.25 per cent per decade (for a perfect site) is taken into account. This lack of statistical significance is confirmed by Figure 7, which shows trends over the whole United Kingdom during the same period. It shows that the variations in trend which occurred in the London area are typical of those over the rest of the country. Urbanization may be expected to yield an increase in the proportion of the rain falling in the summer half-year, and trends of this quantity in the London area

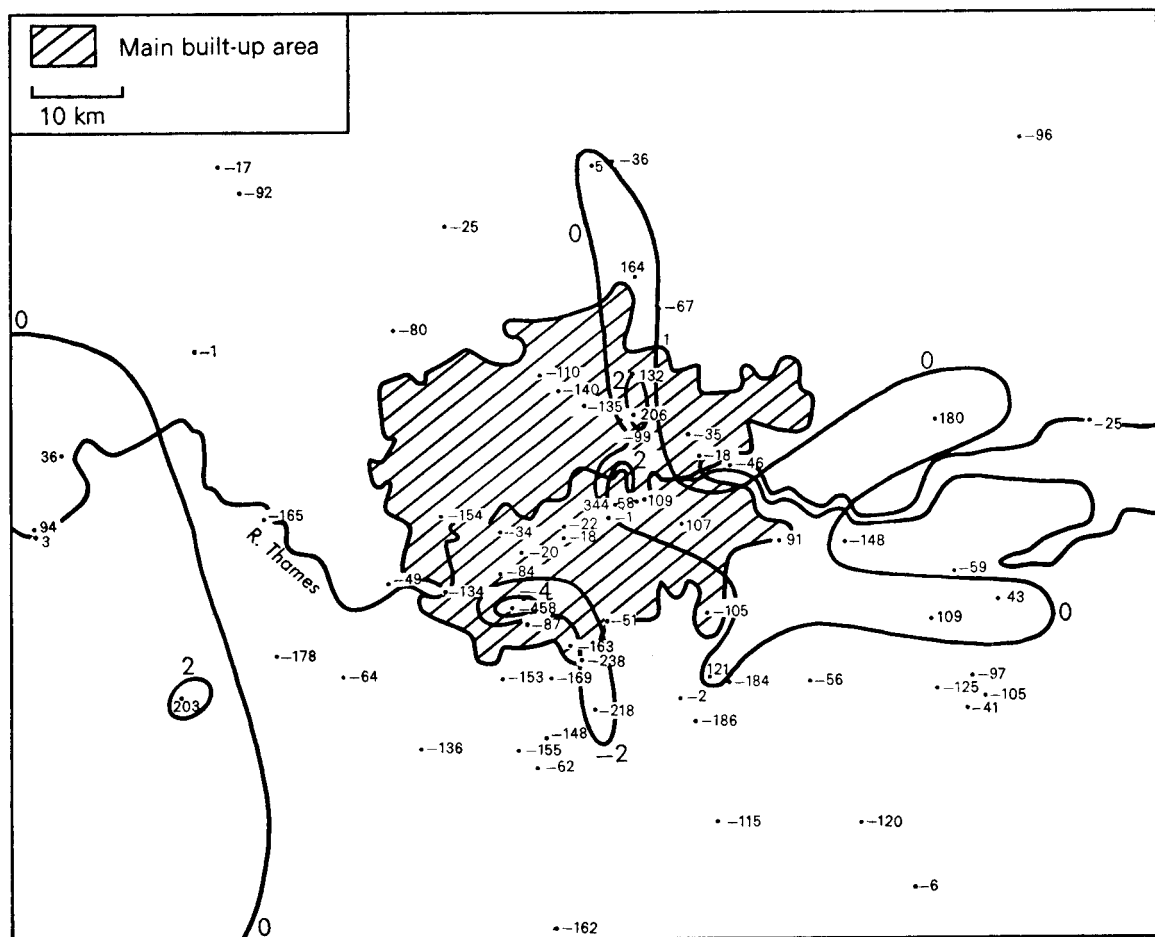


Figure 6. Trend of annual rainfall in the London area (per cent per decade) from 1911 to 1970. Standard error c. 1.25 per cent per decade. Station values are expressed in hundredths of one per cent.

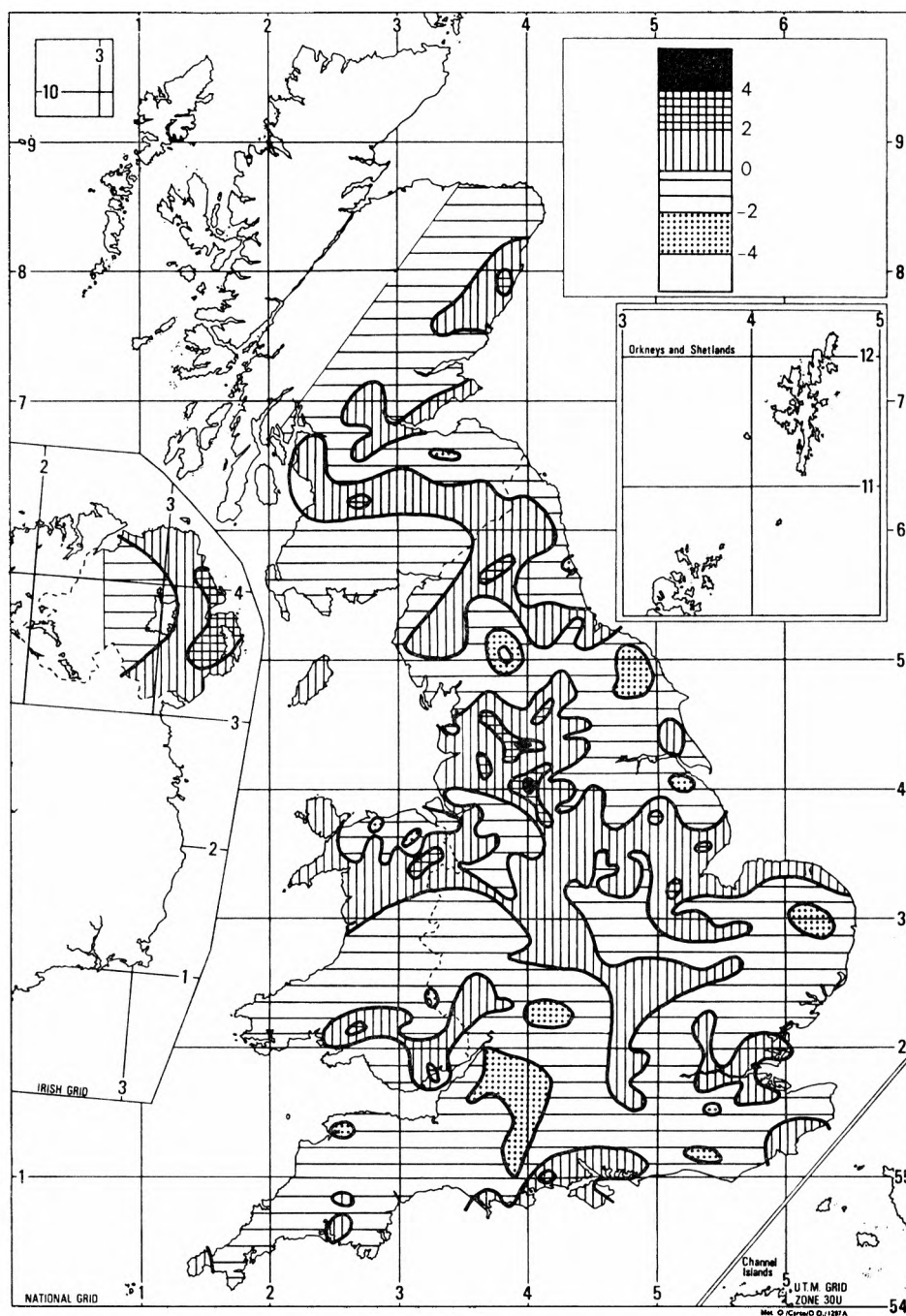


Figure 7. Trend of annual rainfall (per cent per decade) from 1911 to 1970.



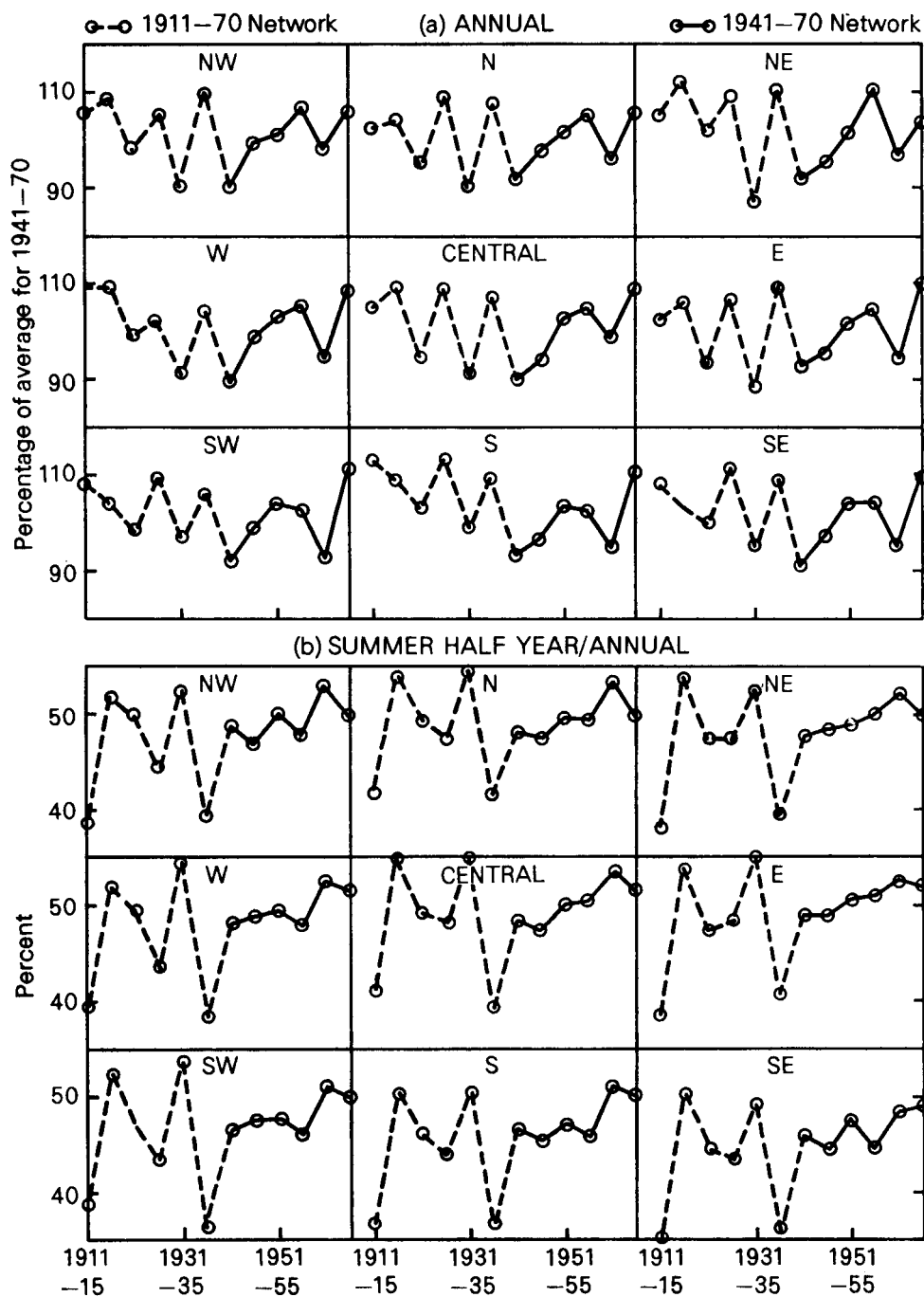


Figure 9. Five-year means of rainfall in the London area.

comparisons can be made between London itself and the area to the south-west. The graphs display a strong spatial coherence, and no features which can clearly be attributed to urbanization are apparent.

## 8. Conclusions

The spatial coherence of trends derived from rain-gauge records was found to be very poor. During the period from 1911 to 1970, values differed by several per cent per decade at points only a few kilometres apart. Errors due to changes of site and instrumentation were found to account for about half the variance of the observed trends.

During the period from 1911 to 1970, no effects of urbanization were revealed in the annual and summer half-year totals of rain in the London area. The investigation was, however, far from exhaustive. The most likely area for effects to be observed is in the frequency of high-intensity, short-duration rainstorms during summer, and this part of the subject was not examined. The main purpose here was to see if any changes in the short-duration rainfall climatology had affected annual and seasonal rainfall totals. On the basis of the data available for the London area, this may be said to be not the case, and homogeneous rainfall series produced without regard to urbanization effects will still be suitable for investigations into climatic change on the annual and seasonal time-scale.

## Acknowledgements

Thanks are due to Mr G. H. Ross for statistical advice, and to Mr I. D. Julien and Miss J. H. Atkinson for programming support. The work was funded by the Department of the Environment.

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551.508.77

## **An improved gravimetric rain-gauge**

By J. R. Sherwood\* and R. E. W. Pettifer

(Meteorological Office, Bracknell)

### **Summary**

A gravimetric rain-gauge (GRG) was developed and tested at Kew Observatory in the early 1970s. Results obtained from this gauge have been promising but it proved difficult to adjust and maintain satisfactorily and had a capacity of only 50 mm of rainfall. The Operational Instrumentation Branch of the Meteorological Office have developed and redesigned parts of the gauge to overcome these difficulties and this paper contains a description of the gauge in its new form.

### **Introduction**

One of the greatest difficulties associated with rainfall measurement is defining the extent to which the gauge affects the flow around it and hence the rainfall it catches (Kurtyka, 1953; Green, 1969). Attempts to overcome this problem fall into two main categories. One method is to shield the gauge from the horizontal component of wind speed by placing metal (Nipher-type) or natural (turf-wall) shields around the gauge. The main drawback of this method is that the shields themselves produce perturbations in the flow. The other method is to sink the gauge into the ground, so that the rim is level with the surface, to reduce its effect on the airflow. This method is not entirely successful because the hole made by the collecting funnel still affects the airflow and there is some insplash into the funnel. Insplash can be reduced by placing a venetian blind or slat arrangement around the gauge but this, too, affects the airflow. The GRG does not overcome these problems but attempts to minimize them. The first GRG was developed at Kew and has been described by Crawford (1972); see also Painter (1975). The gauge had a large collector pan, about 120 cm in diameter with a wire-mesh top surface. A layer of gravel was spread over the mesh and the pan rested on a weighing machine located in a pit so that its top surface was level with the surrounding gravel-covered area. The pan was weighed automatically on a counter platform weighing machine. The counter-balance weight was held in a 'jockey' which was moved along the balance arm by a motor to the position in which the arm was horizontal. Any movement of the arm from the horizontal was detected by one of two photoelectric cells and a positive or negative d.c. voltage was then applied to the motor so as to move the jockey towards the balance position. The position of the jockey along the balance arm was an indication of the accumulated rainfall in the gauge and was monitored by a precision multi-turn potentiometer linked to the motor. The GRG recorded cumulative rainfall automatically and had only a small effect on the airflow over the area. As the gauge and surrounding area had similar surfaces, insplash into and outsplash from the gauge approximately cancelled out. The main drawbacks of the gauge were that it had a rather low capacity (50 mm of rainfall) and that the jockey tended to overshoot the balance position, continuously 'hunting' for the balance position but not finding it. This produced a wide trace on the chart record with the width dependent on the position of two mechanical stops on the balance arm. With very careful adjustment of the stops, the trace could be made quite narrow but for continuous, reliable operation of the gauge, frequent readjustment of the stops was found necessary.

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\* Now at London Weather Centre.

### Modifications to the GRG

The modified gauge is shown diagrammatically in Figure 1, and Plate I shows the gauge before installation. The operational logic of the device is shown in block diagram form in Figure 2. The collector pan has been made smaller, 90 cm in diameter, and lighter; it is now manufactured from fibreglass instead of the original galvanized iron. This reduces the ratio of pan weight to rain weight and with an increase in length of the balance arm to 55 cm allows the capacity of the gauge to be doubled without loss of resolution.

The stability of the system has been increased by slinging the weight below the balance arm and hence moving the centre of gravity of the arm to below the pivot point. A weight of about 50 gm on the pan

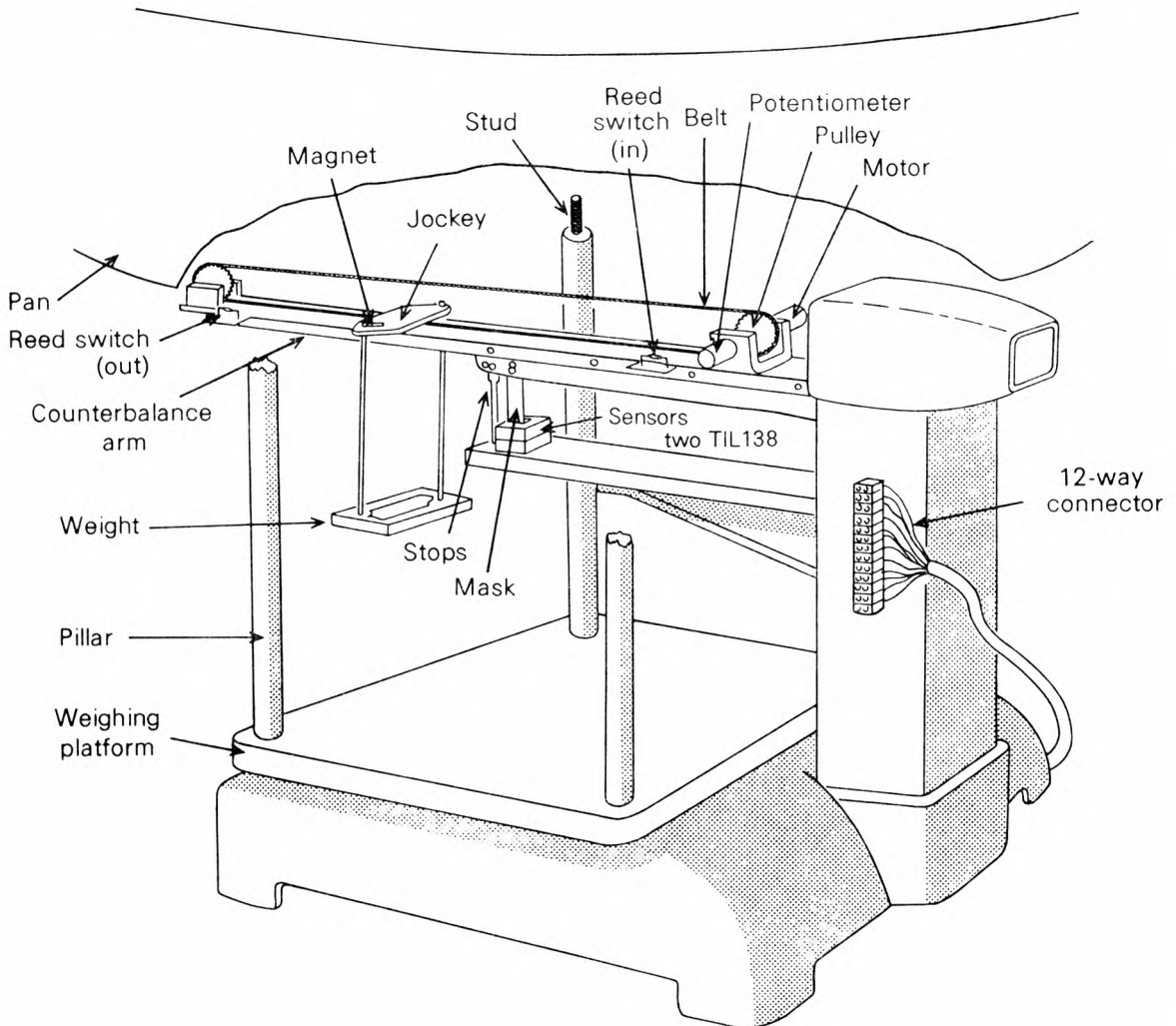


Figure 1. The gravimetric rain-gauge.

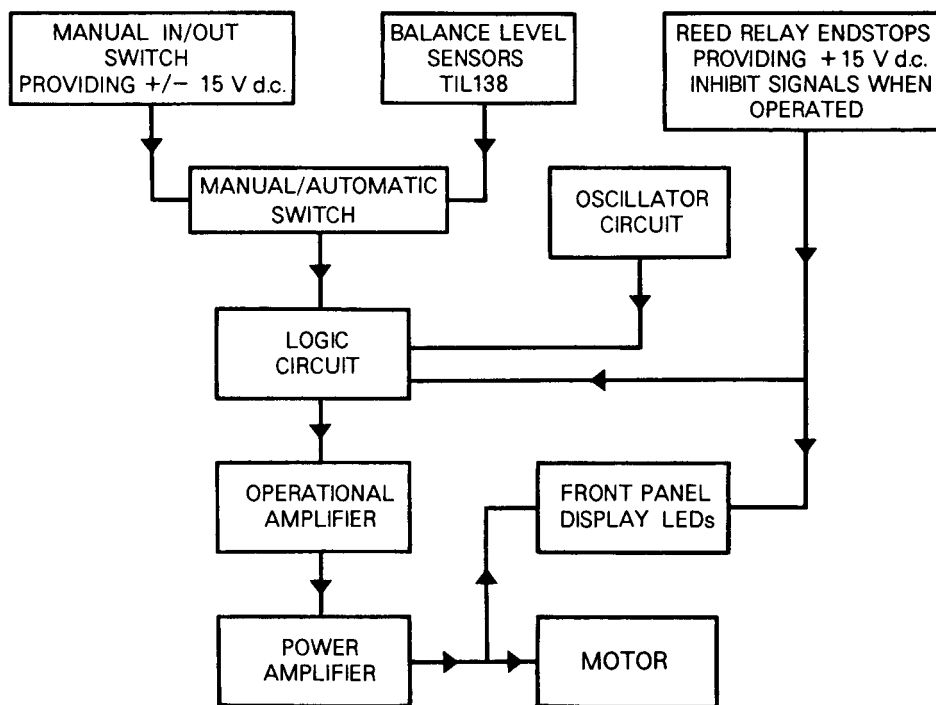




**Plate I.** The gravimetric rain-gauge complete with control electronics.  
The top pan is 90 cm in diameter. (See facing page.)



(a) Motor drive circuit



(b) Potentiometer circuit

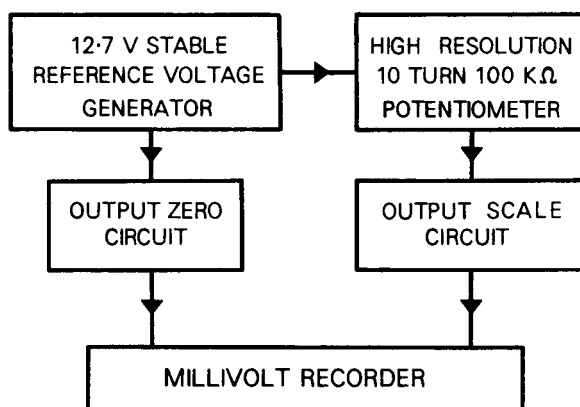


Figure 2. Block diagram of the operation of the modified gravimetric rain-gauge.

causes the balance arm to move between its upper and lower physical stops. The region over which the sensor controls the balance arm is smaller than this, though the actual resolution is limited to some 20 g, equivalent to 0.03 mm of rainfall, by friction in the weighing machine.

### The motor drive

The circuit for the motor drive is shown in Figure 3. Square-wave pulses are generated by an

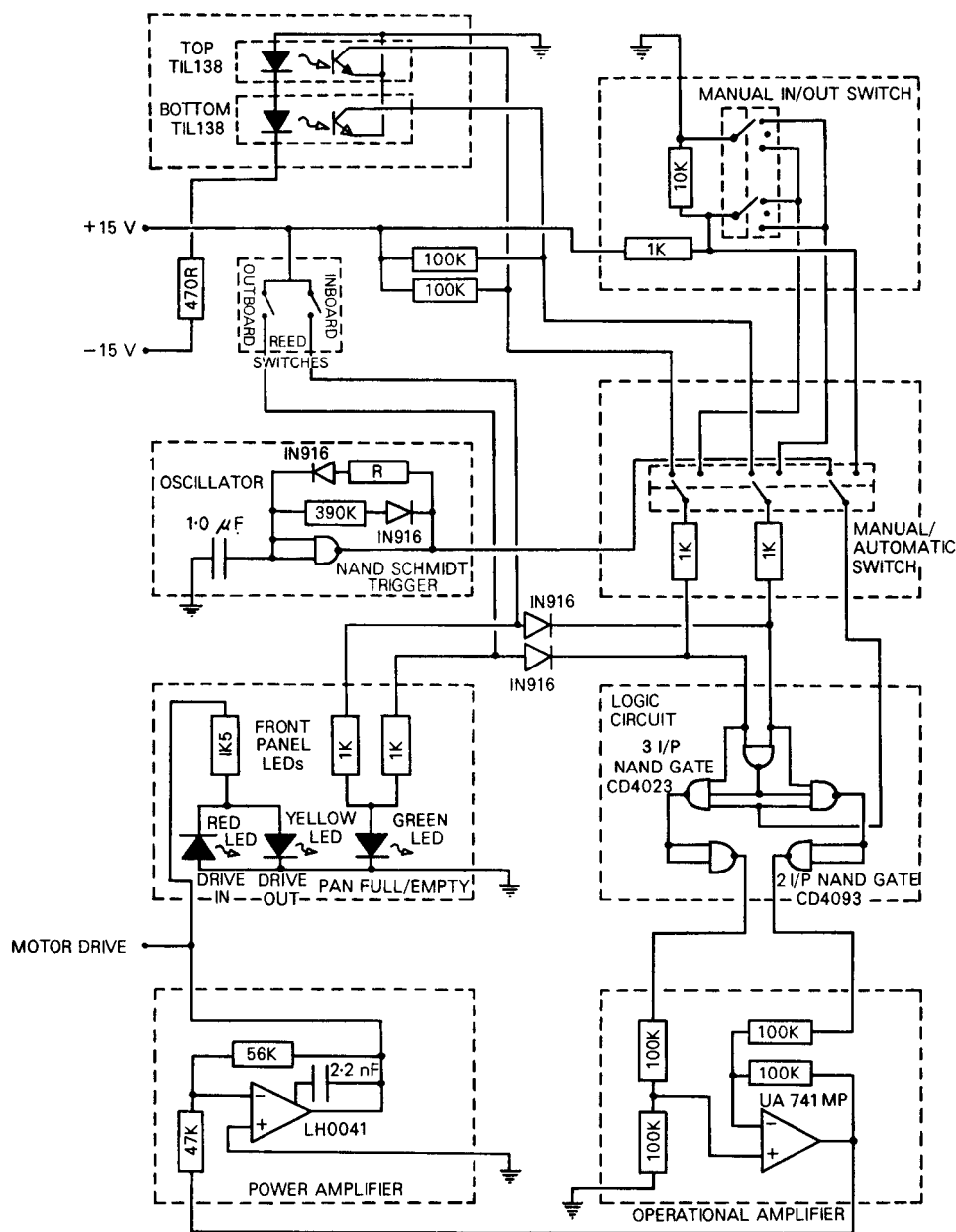


Figure 3. Motor drive circuits.

oscillator circuit using a Schmidt trigger NAND gate. The combination of 390 k $\Omega$  and 1.0  $\mu$ F gives a pulse frequency of 2.56 Hz while the resistor R determines the pulse length. At present the value of R is 39 k $\Omega$  giving a pulse length of 39 ms. This produces a motor drive speed equivalent to a rainfall rate of 155 mm/h. At this speed there is a tendency for the jockey to overshoot the balance position. The motor drive speed can be reduced by substituting smaller values of R, so increasing the likelihood of the balance position being found at the first attempt. However, this introduces the risk that the gauge may not be able to respond rapidly enough to short, intense periods of rainfall. This problem could be overcome by recording rate of rainfall as well as cumulative rainfall and arranging for greater values of R to be switched into circuit as the rainfall rate increases.

On the Kew GRG, the position of the balance arm was sensed by two photosensitive cells operated by torch bulbs. These have been replaced on the modified GRG by a pair of light-emitting diode (LED) and phototransistor devices (TIL 138). The signals from the phototransistors and from the reed relays at each end of the balance arm together with pulses from the oscillator are fed into two inputs of a logic circuit. Governed by the combination of signals it receives, the logic circuit puts pulses on to the positive or negative inputs of an operational amplifier which, in turn, operates the power amplifier which supplies the current to the motor. The reed relays are operated by a magnet fixed to the jockey and, when operated, produce signals which inhibit further movement of the jockey away from the centre of the arm. This does not prevent the jockey from being moved back towards the centre of the arm. If neither phototransistor produces a signal, or if both produce one, the output of the logic circuit remains low and no current reaches the motor.

It is necessary, during calibration or after emptying the collector, to be able to drive the jockey at a fast rate. This is done by replacing the signals from the phototransistors and oscillator on one or other of the inputs to the logic circuit (and hence the motor drive) by 15 V d.c.

### **Control and display facilities**

The switches which provide the choice of manual or automatic drive to the motor are mounted on the front panel of the control box together with 3 LEDs which show whether the jockey is moving 'in' or 'out', or if it has reached one or other of the reed relay stops.

The 0–100 mV range of output from the control box (see below) is equivalent to a range of 0–100 mm of rain and can be displayed or recorded on any suitable millivolt meter, chart recorder or data logger, including the MODLE data logger currently in use within the Meteorological Office.

### **The potentiometer circuits**

The circuit for the measurement of the jockey position is shown in Figure 4. This circuit was designed to provide an output of 0–100 mV for 0–100 mm of rain, which corresponds to the full travel of the jockey along the balance arm, with small adjustments for zero and scale. The position of the jockey along the balance arm, and hence the accumulated rainfall in the gauge, is measured by a high-resolution 10-turn 100 k $\Omega$  potentiometer linked to the motor. The gearing is such that about 5 turns are used. A stable 12.7 V reference voltage is generated by a 6.2 V zener diode with an operational amplifier and is supplied to the potentiometer. It is also used, after reduction by an operational amplifier, to provide a small, adjustable zero offset to the output. The return signal from the potentiometer slider is reduced by an operational amplifier and provides the scale adjustment. The zero and scale outputs provide the low and high sides of the recorded gauge output respectively. They are both effectively isolated from the common ground used throughout the gauge and this reduces noise pick-up and interference.

### **Modified GRG performance**

The gauge was calibrated in the laboratory by adding weights in 5 kg stages up to 60 kg and then



The gauge was installed in the field and the output logged at 1-minute intervals on a Kent chart recorder. From the start the gauge appeared to be very sensitive, easily detecting and showing dew formation at night and evaporation the following morning. Very little rain fell in the first six weeks after installation but the chart record from the gauge compared well with that from a nearby (Dines) tilting-siphon rain-gauge on the few occasions on which rain was recorded. The results of a subsequent comparison between the GRG and a manually read standard five-inch check gauge exposed at 30.5 cm without a turf wall are shown in Figure 5. The latter was read daily and the hourly values taken from the GRG record were totalled to provide equivalent daily figures. From Figure 5 it can be seen that a simple regression line fitted to the data indicates a systematic offset of about 0.10 mm between the gauges and a general increase of about 6 per cent in the catch of the GRG over that of the five-inch gauge. The offset of 0.10 mm is typical of the offset which has been found before in comparisons between the five-inch manually read gauges and 'automatic' gauges. For example, work done to evaluate prototypes of the Meteorological Office Mk 5 tipping-bucket gauge established an offset of between 0.05 mm and 0.15 mm between this gauge and a standard five-inch check gauge. Experiments (unpublished report by B. Tonkinson) have shown that a systematic error of this size can be attributed to the water which remains

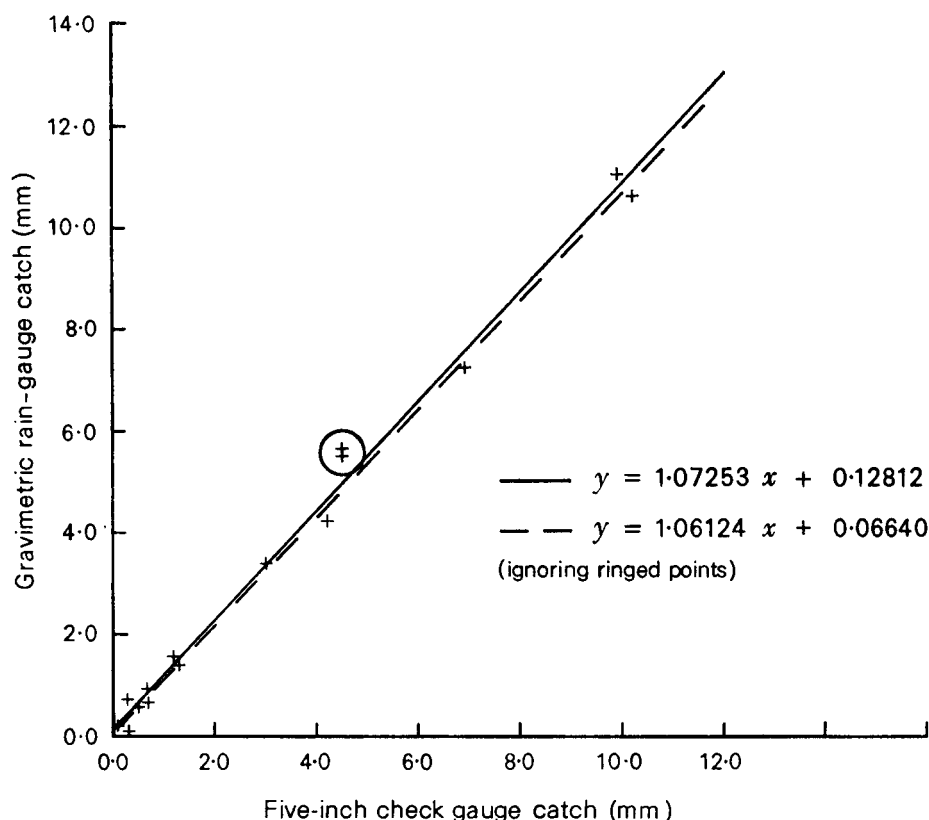


Figure 5. Comparison of catches of gravimetric rain-gauge and five-inch rain-gauge (5 January to 7 March 1979).

in the bottle of the five-inch gauge when it is read and which subsequently either evaporates or is recorded as a 'trace'. The general difference of + 6 per cent between the GRG and the check gauge is consistent with the previously published evidence (Robinson and Rodda, 1969) that a ground-level gauge will catch about 5 per cent more than a well-exposed standard five-inch gauge.

The regression fit gives somewhat smaller values of both slope and offset if the points at 4.5 mm rainfall are excluded. These observations were taken in windy conditions, circumstances which would be expected to decrease the catch of the five-inch gauge and, because of the differential pressure problem discussed below, cause the GRG to over-read.

Two problems came to light after the gauge was installed. The pressure differential between the free atmosphere and the pit underneath the collector pan in which the gauge was located needed to be only 3  $\mu$ bar to produce an apparent weight change equivalent to 20 g, the resolution of the gauge. Hence, even in light winds, the gauge is frequently pushed off the balance position by gusts. This leads to a small scatter of points about the balance position on the chart record. The scatter has so far not exceeded the equivalent of  $\pm 0.05$  mm of rain and it disappeared in calm conditions or when the gauge was shielded from the wind.

The resolution of this instrument is therefore limited at present by wind effects across the measuring pan.

The second problem was that several weeks after installation, the cast-iron weighbridge was found to be severely corroded with rust. The gauge was therefore removed and was thoroughly cleaned and treated with anti-corrosion paint. Insufficient time has elapsed since this treatment was completed for a decision to be made whether this approach to the corrosion problem has been successful or whether more expensive approaches such as the manufacture of the weighbridge from cast alloy or brass will be required.

## Conclusions

The modifications described here have made it possible for the counterbalance mechanism of the GRG readily to find and stay at the balance position when rainfall or evaporation takes place. This reduces wear on the motor and means that the positioning of the mechanical stops on the gauge is no longer critical. All necessary adjustments can be carried out on the electrical circuits in the control box rather than on the gauge in the field. The capacity of the gauge has been doubled without loss of resolution.

The first modified GRG was installed at Beaufort Park in October 1978 and preliminary results have been encouraging. A trial between three such GRGs and other Meteorological Office rain-gauges is planned to last about one year and these results will be published in due course.

## Acknowledgements

The assistance of members of the Operational Instrumentation Branch of the Meteorological Office with this work is gratefully acknowledged.

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- |                 |      |  |
|-----------------|------|--|
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Plate II. The dark band in the sea surface seen from the aircraft at a height of 150 m, viewing to the north-north-west (see facing page).



Plate III. The boundary between the two types of sea surface seen at close quarters during the run at 30 m above sea level, viewing due west (see facing page).

- |                                  |      |   |
|----------------------------------|------|---|
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## **An anomalous condition of the sea surface observed from an aircraft**

By H. Griffiths

(Meteorological Research Flight, Royal Aircraft Establishment, Farnborough)

### **Summary**

A pronounced dark-blue band in the sea surface near the equator was observed from the Meteorological Research Flight Hercules aircraft. The sea surfaces on either side of the band were distinctly different in colour. Aircraft measurements revealed a difference in surface temperature of about 1 °C between the two masses of water.

An unusual feature in the sea surface was observed near the equator (0°26'N, 18°44'W) on 9 September 1979 during the detachment of the Meteorological Research Flight Hercules aircraft to Dakar, Senegal. Two different colours of sea surface were separated by a pronounced dark-blue band a few hundred metres wide orientated roughly east–west and extending to the horizon in both directions. No significant cloud formations were observed in association with the differing sea surfaces, the sky being clear apart from a few patches of small cumulus. Plate II shows the dark band viewed from a distance of a few miles at a height of 150 m. The water just to the north of the band appeared quite turbulent, showing many white crests, although elsewhere the sea was relatively calm. Later in the day the band was seen to branch.

A track was flown approximately at right angles to the band at a height of 30 m. Plate III is a photograph taken from the aircraft just before crossing the band, and illustrates the two distinct shades of sea surface. Both Plates II and III were taken with a hand-held camera viewing from the side of the aircraft. Throughout the run at low level the Barnes radiometer viewing vertically downwards from the aircraft was recording. This instrument is sensitive to radiation in the 11 µm atmospheric window and is used to measure the equivalent black-body temperature of the sea surface from low levels. Figure 1 is a plot of equivalent black-body temperature against time for the run at a height of 30 m, and clearly shows a discontinuity in temperature of about 1 °C between the two areas of sea, the warmer water lying to the north. It can be seen that the rise time of the discontinuity is about 5 s. This is a real feature of the sea surface since the Barnes radiometer has a response time of only 120 ms. Moreover, the field of view of the radiometer is 2°, so at this height and speed areas of sea viewed on successive samples are widely spaced.

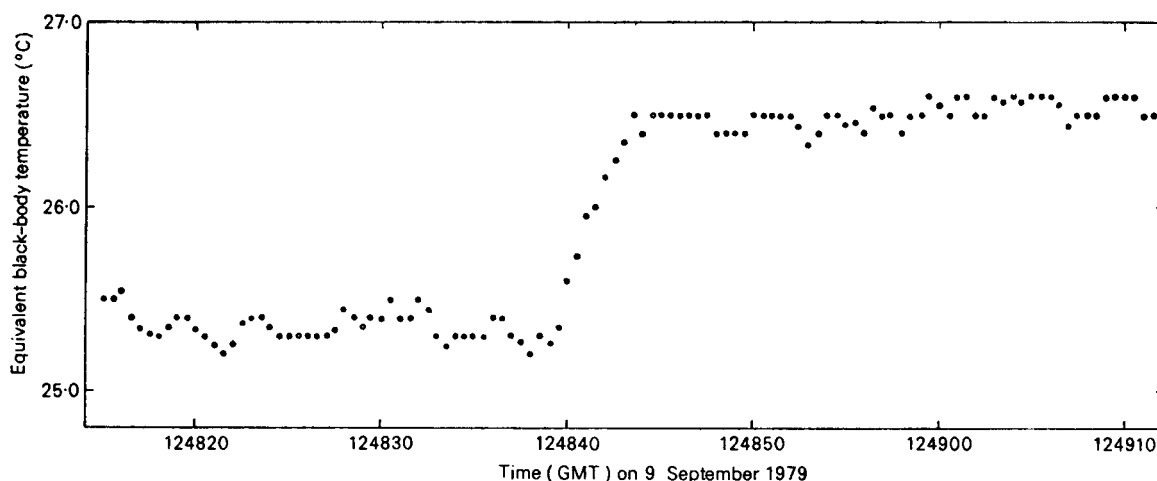


Figure 1. Equivalent black-body temperature of the sea surface as a function of time measured by the Barnes radiometer. The measurements were obtained at a height of 30 m above sea level, with a heading of  $333^\circ$  and a ground speed of  $89 \text{ m s}^{-1}$ .

The wind at this level was  $0.17^\circ/10 \text{ m s}^{-1}$ , that is to say blowing from the warmer to the cooler water. The mean air temperature and specific humidity did not appear to be affected by the change in the temperature of the sea surface, being  $24.6^\circ\text{C}$  and  $14.1 \text{ g kg}^{-1}$  respectively over both regions of sea. However, an analysis of the vertical component of the wind velocity ( $w$ ) measured by the aircraft reveals significant differences between the two regions. The aircraft's wind-finding system is sampled at a rate of 20 Hz thus providing about 500 samples of  $w$  for each region from a total of about 1 minute of data. Figure 2 shows normalized histograms of  $w$  summed in intervals of  $0.06 \text{ m s}^{-1}$  for both regions of sea. A constant was added to all the measured values to set the mean vertical component of the wind velocity over the cooler water arbitrarily to zero. For the second distribution the mean value of  $w$  is only slightly different at  $0.05 \text{ m s}^{-1}$ .

Both distributions are noticeably skewed towards positive  $w$  as would be expected if ascent is accompanied by slower descent over a larger area. We may compare the two distributions using a chi-square test, and this shows the differences between them to be significant at the 0.1 per cent level. Because of this highly significant result it is meaningful to compare the variances of the distributions. For the cooler water the variance of  $w$  is  $0.032 \text{ m}^2 \text{ s}^{-2}$ . However, the corresponding value for the warmer water is  $0.060 \text{ m}^2 \text{ s}^{-2}$  indicating that in this case a  $1^\circ\text{C}$  rise in sea surface temperature has almost doubled the turbulent kinetic energy contained in the vertical component of the wind velocity.

Although the discontinuity in the temperature of the sea surface was particularly well defined, to make any useful inferences about its effect on the boundary layer would require more information than was obtained on this occasion.

It is possible that the sea-surface phenomenon was associated with either the westward South Equatorial Current or the eastward Equatorial Counter-current both of which occur in this part of the ocean at this time of year.

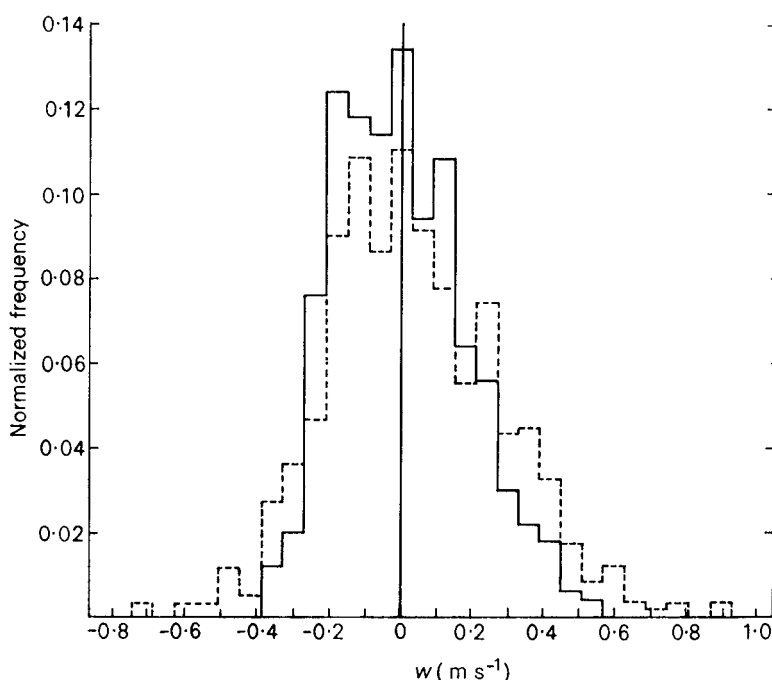


Figure 2. Histograms of the vertical component of the wind velocity ( $w$ ) for the two areas of sea measured at a height of 30 m above sea level.  
 ----- warmer water, 580 samples                      ————— cooler water, 500 samples

## Reviews

*Geophysical fluid dynamics*, by J. Pedlosky. 240 mm × 160 mm, pp. xii + 624, *illus.* Springer-Verlag, Berlin, Heidelberg, New York, 1979. Price DM 79.50, US \$43.80.

Fluid dynamical phenomena abound in the outer layers and interiors of planets and stars. The observational evidence takes very many forms and only in the case of phenomena occurring at or near the Earth's surface, including those with which meteorologists and oceanographers are concerned, have detailed direct observations of flow velocities, pressure, etc. been made over useful periods of time. The term 'geophysical fluid dynamics', which was coined about a quarter of a century ago, has no precise definition, but in its widest sense it is a useful label for the study of basic hydrodynamic (and magnetohydrodynamic) processes underlying fluid dynamical phenomena encountered in the study of planets and stars, including Earth and Sun, as elucidated by theoretical investigations and related laboratory and numerical work. Geophysical fluid dynamics laboratories are now found in many institutions and the use of the prefix 'geo' when objects other than the Earth are involved bothers only the pedants (cf. geometry!).

Good books with misleading or ambiguous titles (e.g. 'Sound reproduction', 'Earth') are not uncommon and this may be one of them. But the title 'Geophysical fluid dynamics' is certainly shorter and more vogueish and eye-catching than 'Some carefully worked mathematical problems in the study of rotating stratified fluids for graduate students with an interest in large-scale motions in the Earth's atmosphere and oceans', which more accurately describes the main contents of this valuable contribution to the literature by a leading worker. The first chapter is a short one giving a good account of the

standard basic dynamic and thermodynamic equations governing the flow of a rotating stratified fluid and introducing basic parameters such as the Rossby (or Kibel) number and the Rossby (or Prandtl) radius of deformation. This is followed by another short chapter presenting several standard theorems governing vorticity, circulation (Kelvin), potential vorticity (Ertel) and relationships governing geostrophic flow (Taylor–Proudman theorem, thermal-wind equation). The rest of the book, apart from a bibliography and index, comprises six long chapters averaging 80 pages in length. The first of these, Chapter 3, entitled ‘Inviscid shallow-water theory’, considers several types of small-amplitude wave motion, including the well-known Poincaré, Kelvin and Rossby waves, introduces Rossby’s useful ‘beta-plane’ for simplifying the equations governing motions on a sphere and discusses resonant interactions associated with weak non-linearity. Dissipative effects enter for the first time in Chapter 4 (‘Friction and viscous flow’), which gives the theory of the Ekman boundary layer, including the important process of boundary-layer suction, and considers applications to ‘spin-down’ and other processes in homogeneous fluids involving frictionally induced changes in potential vorticity as well as the decay of Rossby waves and the structure of side-wall boundary layers when non-linear effects are negligible (Stewartson layers). The southward Sverdrup drift in simplest ‘beta-plane’ models of the wind-driven circulation of the oceans (in which density variations are neglected) is associated with Ekman suction at the surface. The return flow occurs in a highly ageostrophic western boundary current reminiscent of the Gulf Stream in the Atlantic Ocean and the Kuroshio Current in the Pacific Ocean; Chapter 5 presents a systematic treatment of the original theoretical model of this phenomenon, due to Stommel in 1948, and of the many subsequent variations on that theme, including some numerical studies.

Chapter 6 entitled ‘Quasi-geostrophic motion of a stratified fluid on a sphere’ treats a variety of problems bearing on dynamical meteorology and oceanography, including Rossby waves in a stratified fluid, forced stationary waves in the terrestrial atmosphere, theorems governing interactions between waves and zonal flows and the structure of the ocean thermocline. ‘Instability theory’ is considered in some detail in Chapter 7, where the mathematical analysis of incipient baroclinic waves in continuous systems (Eady and Charney) and two-layer systems (Phillips) is given and some effects due to friction and non-linearity are also discussed. Chapter 8, the final chapter, treats ‘Ageostrophic motion’, including continental-shelf waves, theory of frontogenesis and waves in equatorial regions.

The publisher’s claim that this book ‘offers a clear and logical contribution to understanding geophysical fluid dynamics’ is, in this reviewer’s opinion, justified; but only when it is read in conjunction with other important material more closely linked with observations and experiments can the book provide ‘students and scientists with the necessary background for understanding and pursuing research in oceanography and meteorology’.

R. Hide

*Man’s impact on climate. Proceedings of an International Conference held in Berlin, June 14–16 1978. (Developments in Atmospheric Science, Vol. 10),* edited by Wilfrid Bach, Jürgen Pankrath and William Kellogg. 245 mm × 170 mm, pp. xxiv + 328, *illus.* Elsevier Scientific Publishing Company, Amsterdam and New York, 1978. Price US \$53.25, Dfl 120.00.

This book comprises the proceedings of an international conference held in Berlin in June 1978. The conference was organized by the Federal Environmental Agency of the Federal Republic of Germany to ‘document its interest in pressing world problems universally affecting the world community’. It is not surprising therefore that most of the contributors are working in German research institutions.

A summary of the conclusions and recommendations of the conference, including a set of principles for assessing climate impact programs, precedes the main text. This is followed by Part I, 'Climate history, theory and modelling'. It includes interesting contributions from Hasselmann, on the problems of multiple time-scales in climate modelling, and from Wetherald and Manabe, on the sensitivity of a general circulation model with a simple interactive cloud scheme to changes in CO<sub>2</sub> concentration. Part II, 'Mechanisms of Man's impact', considers man-made changes to the gaseous and particulate composition of the atmosphere, and their impact on climate. It also includes papers by Egger and by Jill Williams which assess the effect of waste heat from energy parks on the circulation of the atmosphere. The final section (Part III), 'Potential consequences and the future climate', is concerned mainly with the effect of future energy policies on atmospheric carbon dioxide concentrations, and thence on climate.

All the papers are in English, and have been reproduced using a photographic process. The text and diagrams are clear, though some of the printing is extremely fine, and the labelling on several of the diagrams is microscopic. There are other minor flaws; for example the paper by Eiden on the influence of trace substances was produced on a typewriter with the zero key missing (a lower case letter 'O' is used instead), and Grassl's paper on aerosol particles and changes in planetary albedo includes diagrams which are labelled in German.

This is not a book for the reader with only a casual interest in man's impact on climate. As befits the proceedings of a conference, the topics covered are diverse and most of the papers are technical, and there are other texts which provide a more coherent and readable introduction to the subject. However, some of the contributions, such as those by Flohn (past warm climates), by Zimen and by Hampicke (on the carbon cycle), and by Rotty and by Niehaus (on energy demand and carbon dioxide concentrations), are reasonably digestible and can be recommended as more general reading. There is little that is new to interest the specialist. At best it provides a useful review of various aspects of the subject to date, but at about £30, it is a book to borrow rather than to buy.

J. F. B. Mitchell

## Notes and news

### Kew Observatory

It has been decided with great regret that, as a contribution towards the staff cuts required by the Government, the Meteorological Office station at Kew Observatory will close at the end of this year. The history of the Observatory is recorded in articles in the issues of the *Meteorological Magazine* dated June and July 1969. Since then the National Radiation Centre has been moved from Kew to Beaufort Park and the work at the Observatory has consisted of synoptic and climatological observations, a few specialist measurements (such as atmospheric electricity, evaporation, soil temperature and air pollution) and instrument evaluation. The greatest loss will, of course, be the termination of a climatological record going back over 200 years. Fortunately, the Director of the Royal Botanic Gardens at Kew has kindly agreed to maintain a climatological station, and observations started there on 1 March 1980. Thus climatological records will be continued in the same area.

### **Retirement of Mr Alan Ward**

Mr Alan Ward, Chief Meteorological Officer, London (Heathrow) Airport, retired from the Meteorological Office on 3 May 1980 at the end of a meteorological career spanning 42 years.

Alan Ward joined the Office as an Assistant Grade III in May 1938 following an education in science subjects at the Holywell County School, Wales. After service at aerodromes in eastern England he was selected in 1942 for training as a forecaster and posted to Dyce, near Aberdeen. Promotion to Assistant Grade II at the end of 1942 and commissioning in the Royal Air Force Volunteer Reserve as Pilot Officer in April 1943 led to several further short-lived postings to aerodromes in Britain in locations extending from Scotland to Devon. In the course of these he was promoted to Flying Officer.

In September 1944, Mr Ward was sent as a forecaster to Gibraltar where further promotions to Assistant Grade I and Flight Lieutenant soon followed. He remained at Gibraltar until 1953 with a short break in 1947 for demobilization and reappointment in the new post-reconstruction grade of Experimental Officer. During this period he became expert in dealing with the peculiarly difficult problems associated with the rock and its environs, writing about them in several reports and articles, some of which were published in the *Meteorological Magazine*. These dealt with such diverse topics as sea-breezes, fog, stratus, the flow around the rock, the use of upper-air patterns in forecasting, orographic cirrus and the Blue Sun phenomenon.

Back in England from 1953 Alan Ward pursued his forecasting career at London (Heathrow) Airport, where he was promoted in 1955 to Senior Experimental Officer, and at the Headquarters, Bomber Command, High Wycombe, where in January 1961 he became a Chief Experimental Officer. Soon after, he was appointed to Headquarters, 38 Group at Royal Air Force, Odiham, where he was responsible for setting up and leading a small mobile forecasting team able to go into the field at short notice according to current military need. His close association with the Royal Air Force continued during service as Senior Meteorological Officer in charge of the Main Meteorological Office at Changi in Singapore from 1963 to 1966, a period in that area of some tension which resulted from the policy of confrontation with Malaysia of President Soekarno of Indonesia. Alan Ward's experience and personal qualities fitted him very well for his next appointment as Chief Meteorological Officer of SHAPE on secondment during 1966–69 to NATO. In this post he was commissioned as Group Captain.

In July 1969 Mr Ward's career took an abrupt change of direction when he was selected to lead the non-aviation public service work in this country as Head of Met 0 7a. He filled this post with distinction, his administrative skills, shrewd judgement, clear direction and powers of negotiation all being deployed to best effect. He was largely responsible for the preparation of the *Public Service Handbook* which encapsulated the wealth of procedure and regulations arising in the development of the diverse services provided by the Office for industry, commerce and the general public.

The peak of Alan Ward's career was reached in March 1976 when he became Chief Meteorological Officer at London (Heathrow) Airport on promotion to Senior Principal Scientific Officer. In this post he has been required to steer the Principal Forecasting Office through a difficult and unsettling period. This has involved the planning for a move of the Office with its complex organizational and communications problems and increasing degrees of automation while experiencing constant pressure for economies of both cost and manning.

Alan Ward is one of a large number for whom the Office has unwittingly played matchmaker, having met his wife Margaret during an overseas tour in Gibraltar. Alan and Margaret have contributed much to the social life and the welfare of the offices and communities in which they have found themselves and the writer, amongst many, will remember them for past kindnesses. We wish them a very long and enjoyable retirement; they both plan to be active, with two of their four children yet to complete their education, and to remain at their home in Ascot.

D. H. Johnson





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July 1980

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## NOTICES

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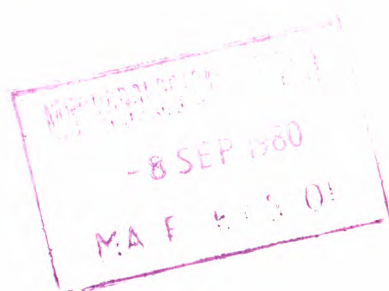
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

**£1.60 monthly**  
Dd. 698260 K15 7/80

**Annual subscription £21.18 including postage**  
ISBN 0 11 722063 9  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

August 1980

Met.O. 931 No. 1297 Vol. 109



# THE METEOROLOGICAL MAGAZINE

No. 1297, August 1980, Vol. 109

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## **An aircraft-mounted pyranometer**

By A. P. Cluley and J. P. Cowley

(Meteorological Office, Bracknell)

### **Summary**

The installation of an upward-facing pyranometer on the Hercules aircraft of the Meteorological Research Flight is described. High-quality data have been obtained which permit detailed study to be made of both the horizontal and vertical variations of solar irradiance. As an illustration, data are presented from a flight on an occasion of a marked thermal inversion. Values of the turbidity coefficient proposed by Unsworth and Monteith are derived and the horizontal and vertical variations in these are examined.

### **Introduction**

The ability to measure solar irradiances from an aircraft allows the spatial distribution of solar irradiance to be investigated in a variety of meteorological conditions. It also allows the contribution that the absorption of solar energy makes to the heat budget of clouds (e.g. stratocumulus) and that of the boundary layer to be estimated.

In this paper, the installation of an upward-facing pyranometer on the Hercules aircraft of the Meteorological Research Flight (MRF) is described. Some illustrative results from this instrument above and below a haze layer are presented only to demonstrate that the equipment is capable of giving measurements of high quality and of yielding some interesting results. Other radiation instruments (downward-facing pyranometer, upward- and downward-facing pyrgeometers) are currently being tested on the aircraft but these will not be discussed further here.

Pyranometers have been installed on earlier MRF aircraft (Roach, 1961) but the present installation yields more accurate results more readily as an instrument of superior performance has been used and the data from it are recorded on magnetic tape thus enabling subsequent computer processing.

### **General description and performance**

An Eppley pyranometer (model PSP) has been installed on the top of the fuselage between the wings of the Hercules aircraft; see Plate I which is a photograph of the installation. This instrument is sensitive to radiation in the wavelength range 0.3 to 3  $\mu\text{m}$  and has a time-constant of about 1 second.

It is mounted on a platform which is approximately level with respect to the aircraft reference axes and the whole assembly is exposed to the airflow during flight. A protective cover is fitted over the dome of the instrument when the aircraft is on the ground or during flights for which the instrument is not required. Exposure is good, although two parts of the aircraft, the tail and the radar pod, have had to be obscured from its field of view by installation of two matt-black obscurers. The effect of these on measurements is discussed later.

The output from the instrument is hard-wired into a head amplifier contained in a unit just inside the aircraft skin near the instrument. This unit also contains the electronics associated with a thermistor which is used to monitor the temperature of the instrument housing. The signal and temperature are sampled at  $1 \text{ s}^{-1}$  and  $0.25 \text{ s}^{-1}$  respectively. The gain and zero offset of the head amplifier can be monitored in flight by switching two precision voltages across the input in place of the pyranometer output. The instrument and electronics have performed well and the intrinsic noise level in the system is less than  $\pm 1$  bit on the data recording system ( $= \pm 0.7 \text{ W m}^{-2}$ ). The gain and zero offset of the head amplifier are very stable though they are slightly temperature dependent, the gain varying by less than 1 per cent over the range of temperature ( $-40^\circ\text{C}$  to  $+20^\circ\text{C}$ ) experienced under normal operating conditions.

The sensitivity of the instrument itself is dependent on its temperature but as this is measured, a correction can be applied. However, misleading results can be obtained under non-equilibrium conditions when the temperature of the instrument is changing.

### **Determination of radiation flux density**

To determine radiation flux densities (irradiances) the absolute calibration, its dependence on instrument temperature and the calibration of the electronics must be known.

The instrument was originally calibrated in December 1975 by the manufacturer at a temperature of  $25^\circ\text{C}$  by comparison with standard instruments which can be referred to the International Pyrheliometric Scale 1956, and it is this calibration that is used in this paper. Future calibrations will be carried out annually by the Meteorological Office at Bracknell, where outdoor comparisons with a standard pyrheliometer will be made.

To study the effect of temperature on the sensitivity of the instrument, it was exposed to a constant source of light from a xenon arc lamp and its output monitored as the temperature was changed. However, to obtain meaningful results a number of points had to be watched:

(1) The stability of the lamp output was checked and was found to vary by less than 0.5 per cent after an initial warm-up period of two hours.

(2) The optical arrangement was such that a uniform beam of light was incident on the instrument. Hence small variations in alignment caused by temperature changes could not significantly affect the instrument output.

(3) The instrument and head amplifier were housed in a vacuum chamber to prevent the optics being degraded by condensation.

(4) The gain and offset of the head amplifier were regularly measured throughout the experimental runs.

(5) Readings were taken only during periods when the temperature of the instrument was stable.

The results are illustrated in Figure 1, where the output voltage corrected for amplifier drifts is plotted against instrument temperature. Each run took a day. The arrows indicate the direction of temperature change between the individual measurements. Most of the scatter in this diagram probably arises from fluctuations ( $\approx 0.5$  per cent) in the lamp output during individual runs and differences in

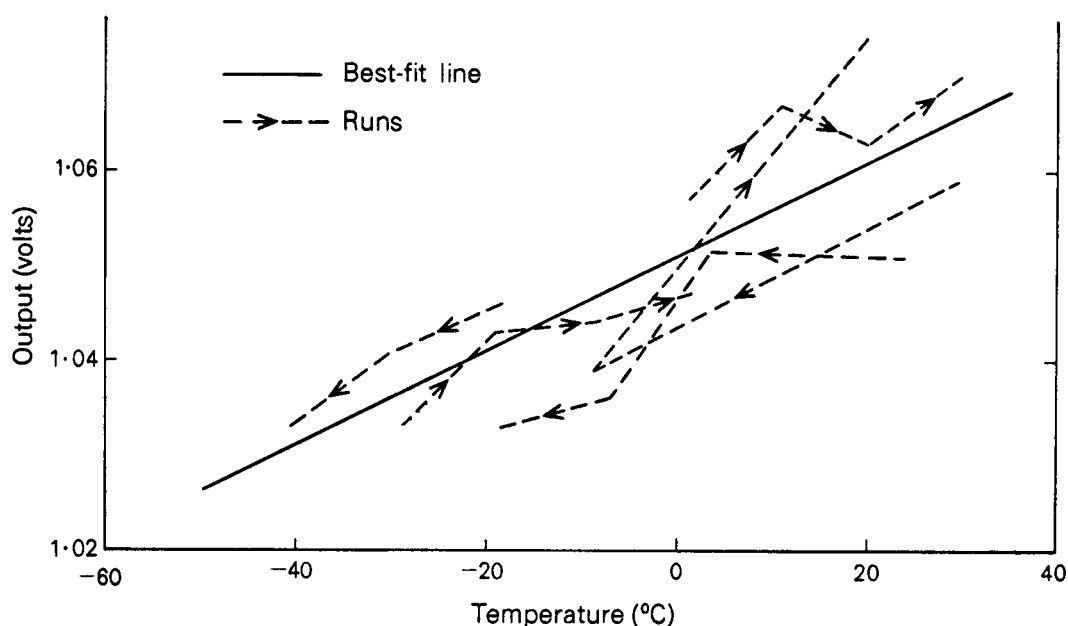


Figure 1. Variation of pyranometer output with instrument temperature.

its mean output from run to run. However, a clear trend emerges and the best-fit line for the variation of the output of the instrument with temperature is illustrated. This may be transformed to give the instrument sensitivity relative to the value at 25 °C (the manufacturer's calibration temperature):

$$S = S_{25}\{1 + \gamma(T - 25)\},$$

where  $S$  ( $\text{V W}^{-1} \text{m}^2$ ) is the sensitivity at  $T$  °C,  $S_{25}$  is the sensitivity at 25 °C and  $\gamma = 0.00047 \text{ K}^{-1}$ . There is no systematic difference between the measurements taken when the temperature was increasing and those taken when it was decreasing.

### Interpretation of measurements

We now consider the effect of the obscurers. The solid angles subtended at the detector by the fin and radome obscurers are 0.10 and 0.04 sr respectively (i.e. 2.2 per cent of the field of view has been obscured). However, if the radiation field is isotropic, then by use of the cosine law it is estimated that about 99 per cent of the true amount of radiation will be incident on the detector. Thus if the incident radiation is predominantly diffuse, the effect can be removed by multiplying the data by a factor of 1.01. However, if it is predominantly direct, then no correction is necessary for the effect of the obscurers.

In level flight the aircraft flies with its nose up, so the inclination of the instrument with respect to the sun depends on the aircraft heading. This was investigated by flying six runs at 60° intervals in heading at a height of 3 km in cloudless conditions. The pyranometer output was averaged over two-minute runs (i.e. 120 samples) and these results are presented in Table I. An expression can be derived

**Table I.** *Dependence of pyranometer output on azimuth of sun relative to aircraft heading*

Aircraft heading	Sun's azimuth	Pyranometer output	Corrected output
	<i>degrees</i>	<i>arbitrary units</i>	
227	182	1330.5	1332.6
167	183	1316.6	1330.3
106	185	1327.0	1335.7
49	186	1335.2	1332.6
348	188	1344.4	1330.3
286	190	1350.9	1340.8

to enable the instrument output to be corrected for the inclination of the instrument to the horizontal. The derivation assumes all the incoming radiation is direct (greater than 90 per cent in the present case) and that the inclination to the horizontal is small and leads to a correction factor

$$F = \frac{(1 + \tan^2 \alpha + \tan^2 \beta)^{\frac{1}{2}}}{1 + \tan \alpha \tan z \sin(\phi - \theta) - \tan \beta \tan z \cos(\phi - \theta)}, \quad \dots \quad (1)$$

where  $\alpha$  is the inclination of the instrument in a vertical plane normal to the direction of movement of the aircraft (roll angle, positive for left wing up),

$\beta$  is the inclination of the instrument in a vertical plane containing the direction of movement of the aircraft (pitch angle, positive for nose up),

$\theta$  is the aircraft heading,

$z$  is the solar zenith angle, and

$\phi$  is the azimuthal angle of the sun.

Two equations containing  $\alpha$  and  $\beta$  may be obtained by use of data from the first four runs; solving gives

$$\alpha = -0.6^\circ \quad \text{and} \quad \beta = 0.8^\circ.$$

During these runs the aircraft's inertial navigation system indicated a mean aircraft roll angle of  $-1.1^\circ$  and a mean pitch angle of  $3.1^\circ$ . Hence there must be offsets,  $0.5^\circ$  in roll and  $2.3^\circ$  in pitch, between the inclination of the pyranometer sensor and the reference levels of the inertial platform.

In Table I, corrected values are included which were derived using equation (1) and the values of  $\alpha$  and  $\beta$  obtained above. The spread in these corrected values (ideally zero) is less than 1 per cent which is a third of that before corrections are applied. The residual spread is probably due to inaccurate determination of  $\alpha$  and  $\beta$  and the fact that roll and pitch angles were not constant along each run. For flights in which the incident radiation is predominantly direct this correction factor may be used with the measured aircraft roll and pitch angles, incorporating the offsets derived above, to estimate the irradiance through a horizontal surface. If the radiation is mainly diffuse, no correction should be made.

### Experimental flight

The first mission flown by the aircraft with the specific task of measuring global solar irradiance was carried out on 20 September 1977. Data were recorded for a series of straight and level runs in an area to the west of the Cornish coast, centred at  $50^\circ 29' \text{N}$   $6^\circ 06' \text{W}$ . There was a 4 K inversion (see Figure 2) at a height of 1.8 km (830 mb), this level being well marked by the top of a haze layer. The area was cloud free apart from a few cumulus clouds which affected measurements on runs 2, 3 and 9.

A series of 10 runs was flown at 165 m above mean sea level (1015 mb), this being the lowest altitude



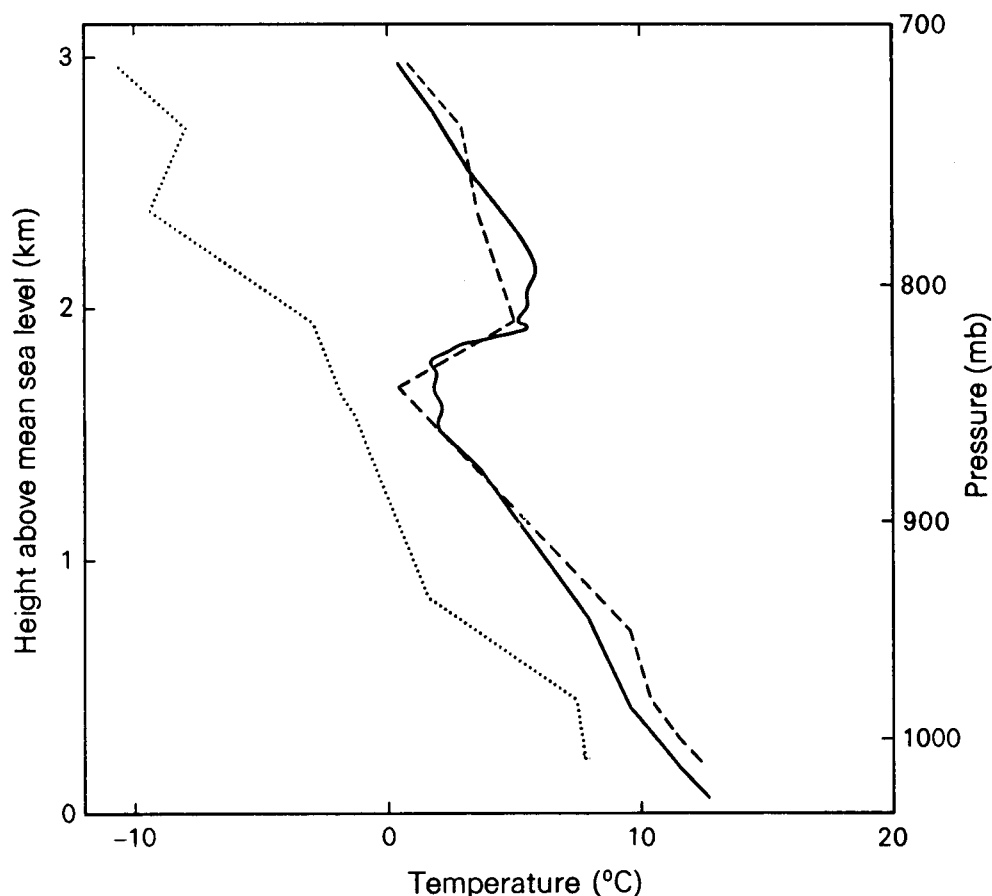


Figure 2. Temperature and humidity profiles on 20 September 1977.  
 - - - - - Camborne 12 GMT dry-bulb temperatures.  
 ..... Camborne 12 GMT dew-point temperatures.  
 — Aircraft temperatures.

considered safe for prolonged flight, and the pattern was repeated at 2060 m above mean sea level (789 mb), which was well above the inversion. The flight pattern is shown in Figure 3.

### The effect of the inversion

The two diagonal runs (i.e. Nos. 1 and 11), details of which are summarized in Table II, have been chosen to illustrate the difference in the transmission of solar radiation above and below a well-defined inversion. The measurements of global irradiance for these runs are shown in Figure 4 before and after the application of equation (1) to correct for changes in the attitude of the aircraft, assuming

(a) roll and pitch angles measured by the aircraft's inertial platform, when corrected by the offsets derived above, refer to the plane of the sensing surface of the pyranometer, and

(b) the global radiation is predominantly direct.

It is suggested later that assumption (a) is not entirely correct. Figure 5 shows the standard deviation

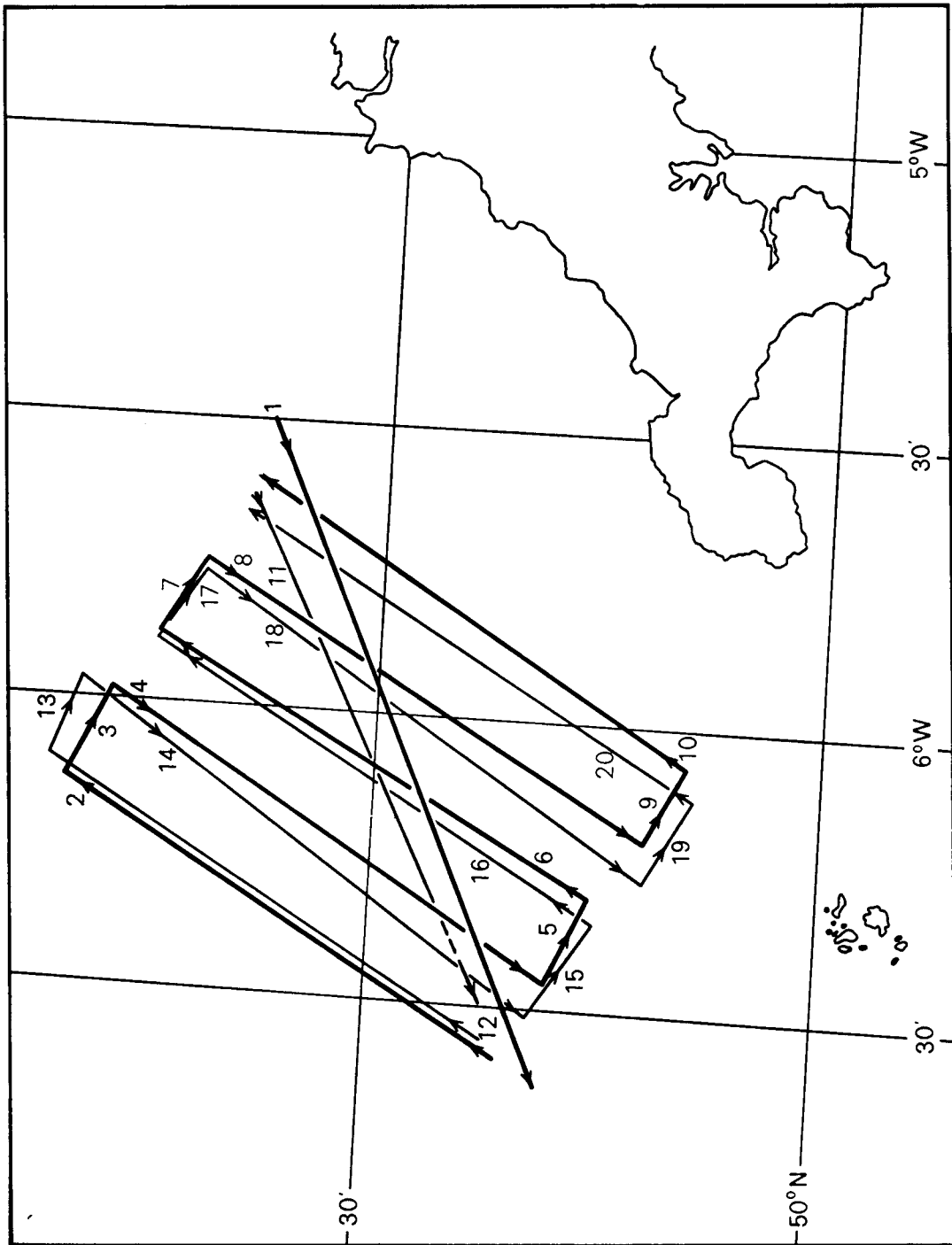
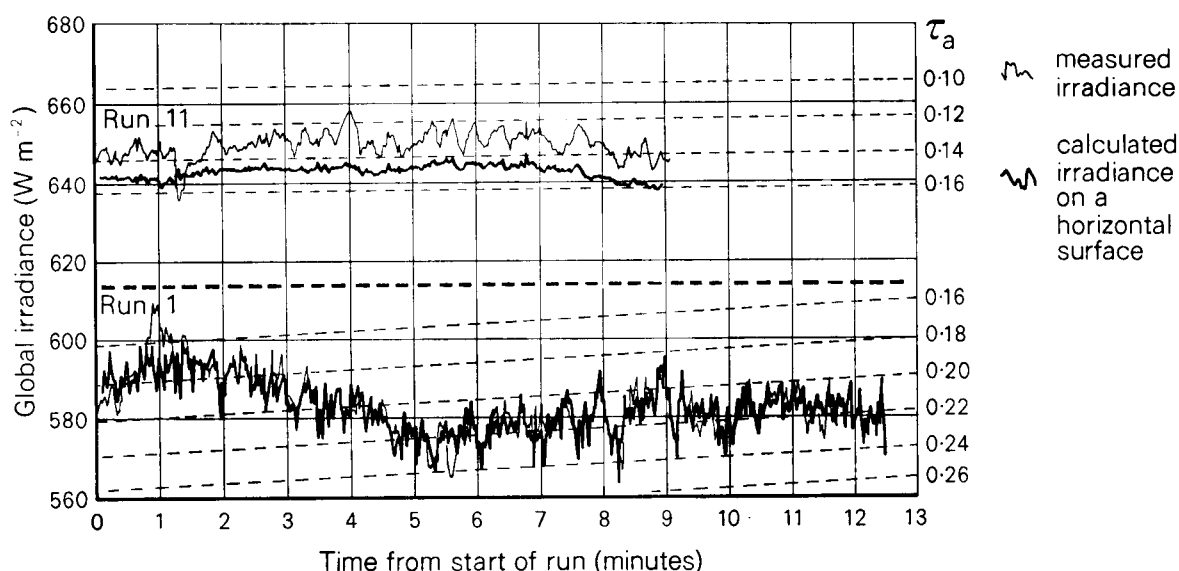


Figure 3. The flight pattern on 20 September 1977. Thin lines—high level (2060 m); thick lines—low level (165 m).

**Table II.** *Flight parameters for runs 1 and 11, 20 September 1977*

	Run 1	Run 11
Start time (GMT)	1118	1310
Finish time (GMT)	1130	1319
Height above mean sea level (m)	165	2060
Height (mb)	1015	789
Start position	50°40'N 5°20'W	50°40'N 5°20'W
Finish position	50°20'N 6°30'W	50°20'N 6°30'W
Start zenith angle (degrees)	50.5	52.1
Finish zenith angle (degrees)	49.7	51.9
Mean aircraft heading (degrees)	243	243
Mean aircraft pitch angle $\pm$ standard deviation (degrees)	2.1 $\pm$ 0.1	1.5 $\pm$ 0.1
Mean aircraft roll angle $\pm$ standard deviation (degrees)	0.4 $\pm$ 0.4	0.4 $\pm$ 0.3
Wind at aircraft height	090°/5 m s <sup>-1</sup>	065°/6 m s <sup>-1</sup>



**Figure 4.** Global solar irradiance on a horizontal surface for runs 1 and 11.

of the data about linear trends for 30-second sections along each run, comparing the variation in the data before and after applying equation (1). It is clear from the diagram that changes in the aircraft's attitude are largely responsible for the fluctuations in the measured irradiances above the inversion, but rapid fluctuations remain in the data from below the inversion. It is suggested here that this is because

(a) the proportion of diffuse component in the global radiation at lower levels is larger, causing the correction for the aircraft's attitude (which assumes there to be only the direct component) to be less accurate, and

(b) there are small-scale variations in the turbidity of the air trapped in the haze layer.

The latter cause is thought to be the major one. The fluctuations in the data for run 11 after correction for the aircraft's attitude give a standard deviation of less than 1 W m<sup>-2</sup>, which is the limit of resolution of the measuring system.

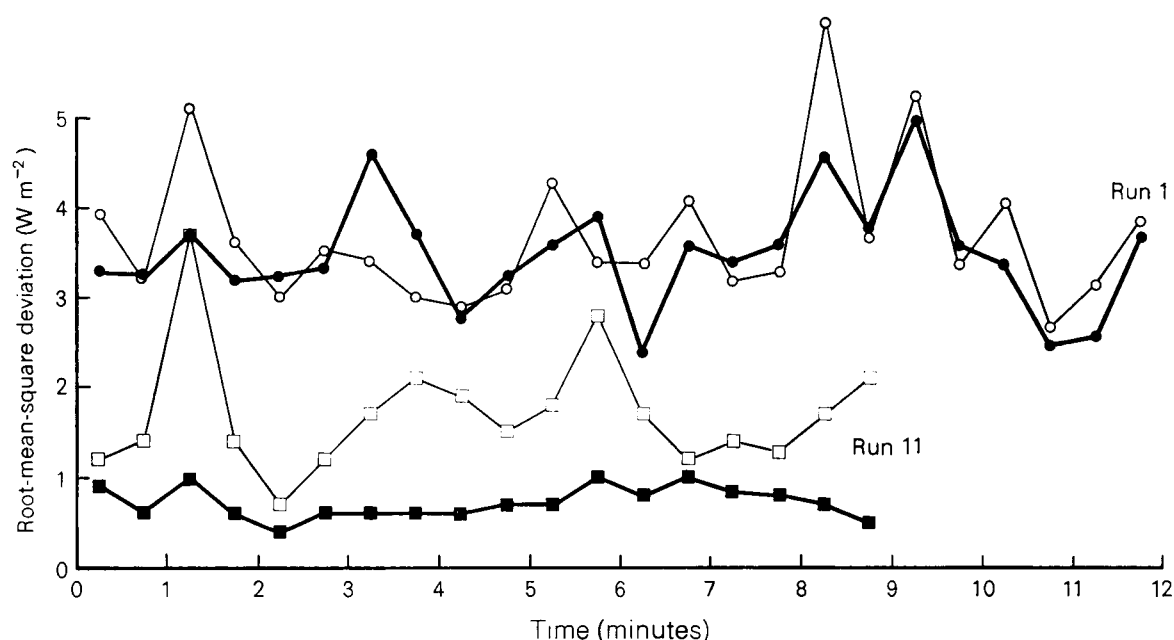


Figure 5. Root-mean-square deviation of irradiance for 30-second sections for runs 1 and 11. Each plot is the r.m.s. deviation of data about the linear trend over 30 seconds.

○ □ data not corrected for attitude of aircraft.  
● ■ data corrected for attitude of aircraft.

Even in the absence of cloud the atmospheric components—air molecules, water vapour, aerosols—scatter a proportion of the direct solar beam into a diffuse component which is received in combination with the remaining unscattered solar beam by a global radiation pyranometer. It is not easy to calculate directly the extent of the absorption of the solar beam from global radiation measurements; instead we use here an empirical model developed by Souster, Rodgers and Page (1978) from which the global irradiances can be estimated for a known precipitable water content, solar zenith angle (which determine the air and water vapour mass along the beam) and turbidity. The turbidity coefficient used in this model is the  $\tau_a$  proposed by Unsworth and Monteith (1972). They defined  $\tau_a$  by

$$S(\tau_a) = S(0) \exp(-\tau_a m)$$

relating the measured flux at normal incidence  $S(\tau_a)$  to the flux  $S(0)$  calculated for a dust-free (but moist) atmosphere, when the optical air mass is  $m$  ( $m \approx \sec z$ ). This turbidity coefficient is therefore a measure of the aerosol loading in the atmosphere.

Figure 4 shows isopleths of constant  $\tau_a$  deduced from values of global radiation (corrected for the orientation of the pyranometer) using the model assuming values of precipitable water content calculated from the 12 GMT radiosonde ascent made from Camborne. Figure 6 shows the values of  $\tau_a$  for each 30-second period of the two diagonal runs. It is evident that  $\tau_a$  varied considerably during run 1 (below the inversion) ranging from 0.17 to 0.22, but there was much less change during run 11 (above the inversion) where  $\tau_a$  was rather smaller, and only varied slightly between 0.15 and 0.16. It is suggested later that these longer-term changes in  $\tau_a$  are correlated vertically and therefore are not a consequence of the inversion.

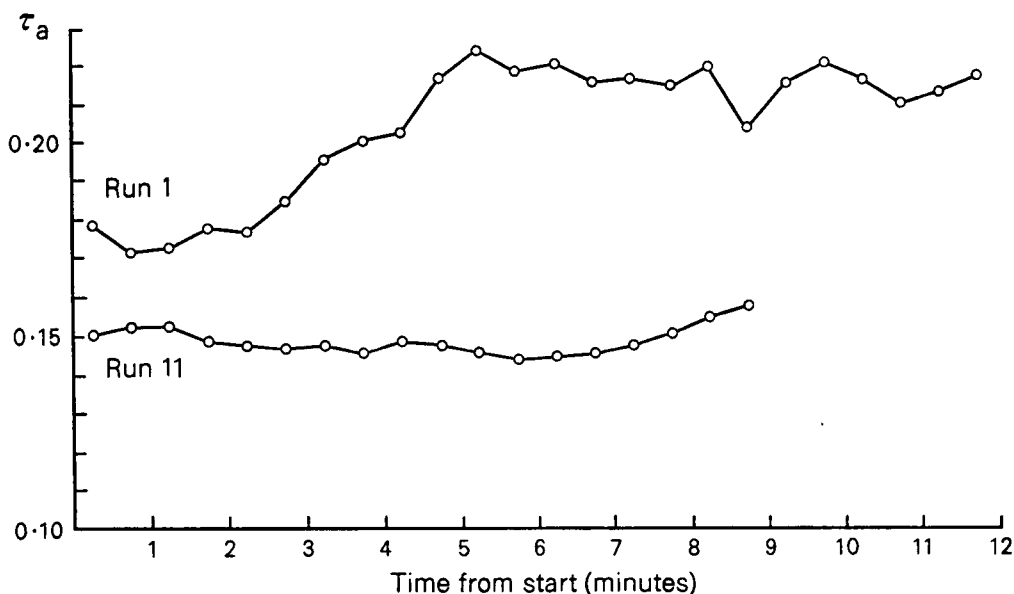


Figure 6. Turbidity coefficients  $\tau_a$  for 30-second sections (runs 1 and 11).

It has been assumed that the precipitable water content of the atmosphere was sensibly constant during the experiment and that the variations of the irradiance were not a result of changes in the amount of precipitable water. These are valid assumptions because:

(a) The transmission of short-wave radiation through the atmosphere is insensitive to water vapour, a change of 1 mm in precipitable water altering the global irradiance by only  $2 \text{ W m}^{-2}$ .

(b) Marked changes in precipitable water only occur near frontal zones and there were none near the experimental area.

The effect of the inversion may be summarized to be:

(a) Global irradiances measured below the inversion are smaller (by about 20 per cent) than those measured above the inversion with corresponding differences of the reverse sign in the inferred turbidities.

(b) The existence of large-amplitude, high-frequency fluctuations in global irradiance (and hence in  $\tau_a$ ) below the inversion.

### Comparison of low-level runs

In order to determine whether, at a fixed height, the longer-term changes in turbidity described above are spatial or temporal, values of  $\tau_a$  have been estimated for runs 2, 4, 8 and 10 (run 6 was excluded because the data were unaccountably corrupted).

Figure 7 shows the variations in 30-second average turbidities for the four runs. The direction of time in the diagram depends on the run, the southern end of each run being to the left. Values of  $\tau_a$  are not shown for sections in which data showed too great a variability to make a value of  $\tau_a$  meaningful. It is evident that  $\tau_a$  decreases going northwards along each run and that there is a notable minimum about 1 minute from the northern end of each run. It appears, therefore, that there is a spatial correlation from run to run in the longer-term variation in  $\tau_a$ .

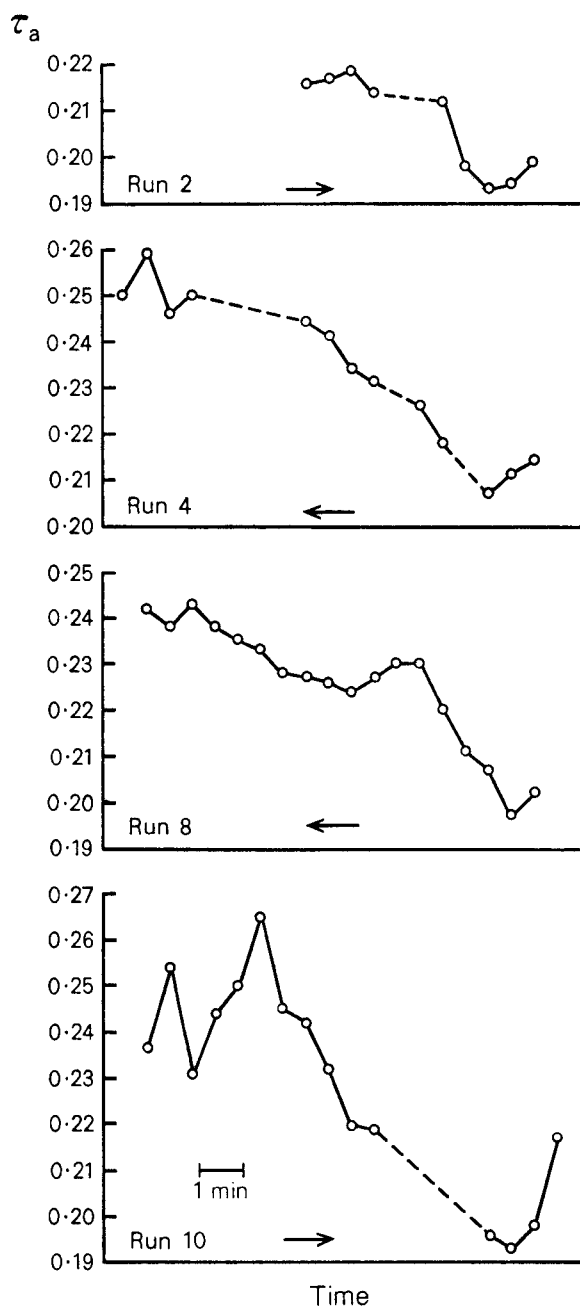


Figure 7. Turbidity coefficients  $\tau_a$  for 30-second sections (runs 2, 4, 8 and 10).

### Comparison of high-level runs

The variation in  $\tau_a$  along the runs at the higher level is shown in Figure 8. A pattern is detectable among the runs but there is some variation from run to run in the mean value of  $\tau_a$ , southbound runs having higher values. It is thought that this is due to the inexact derivation of the offsets between the orientation of the pyranometer sensor and the inertial platform reference levels. An error in the offsets of  $0.3^\circ$  could explain the discrepancy apparent in Figure 8, particularly as, above the inversion, the deduced values of  $\tau_a$  are very sensitive to the absolute magnitude of the irradiance. Whilst the absolute magnitude of  $\tau_a$  is in doubt, changes of  $\tau_a$  along a run can be studied.

Runs 12 and 14 show  $\tau_a$  decreasing northwards, while runs 18 and 20 show an increasing fall-off to the south. Therefore, even above the inversion there appears to be moderate spatial correlation in the turbidity. This implies that the lower frequency variation in  $\tau_a$  (which is a measure of atmospheric aerosol only) is not a consequence of the inversion, but is also a feature of a greater depth of the atmosphere, caused perhaps by remote sources of aerosol. The air mass being studied here had travelled across the Midlands and the Severn Estuary (including industrial plants that generate plumes rich in aerosol, such as sulphur particles).

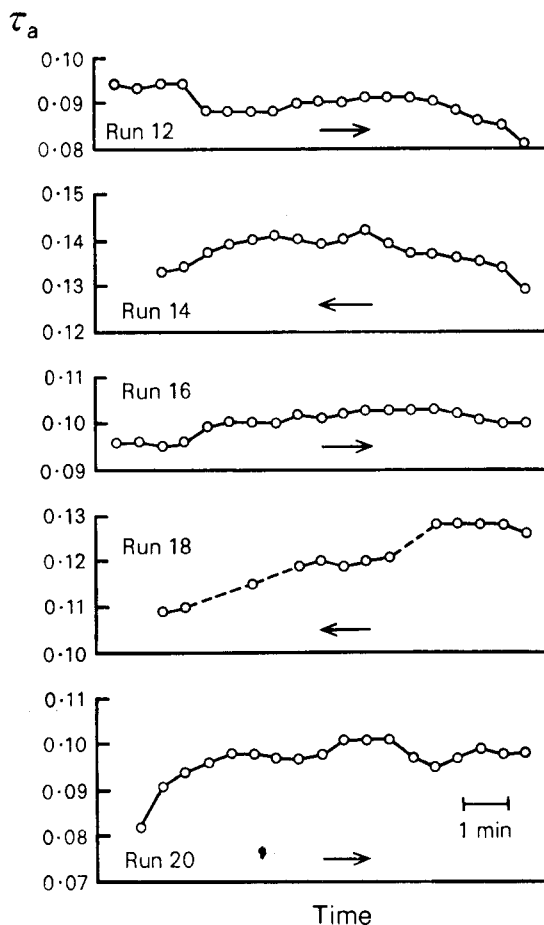


Figure 8. Turbidity coefficients  $\tau_a$  for 30-second sections (runs 12, 14, 16, 18 and 20).

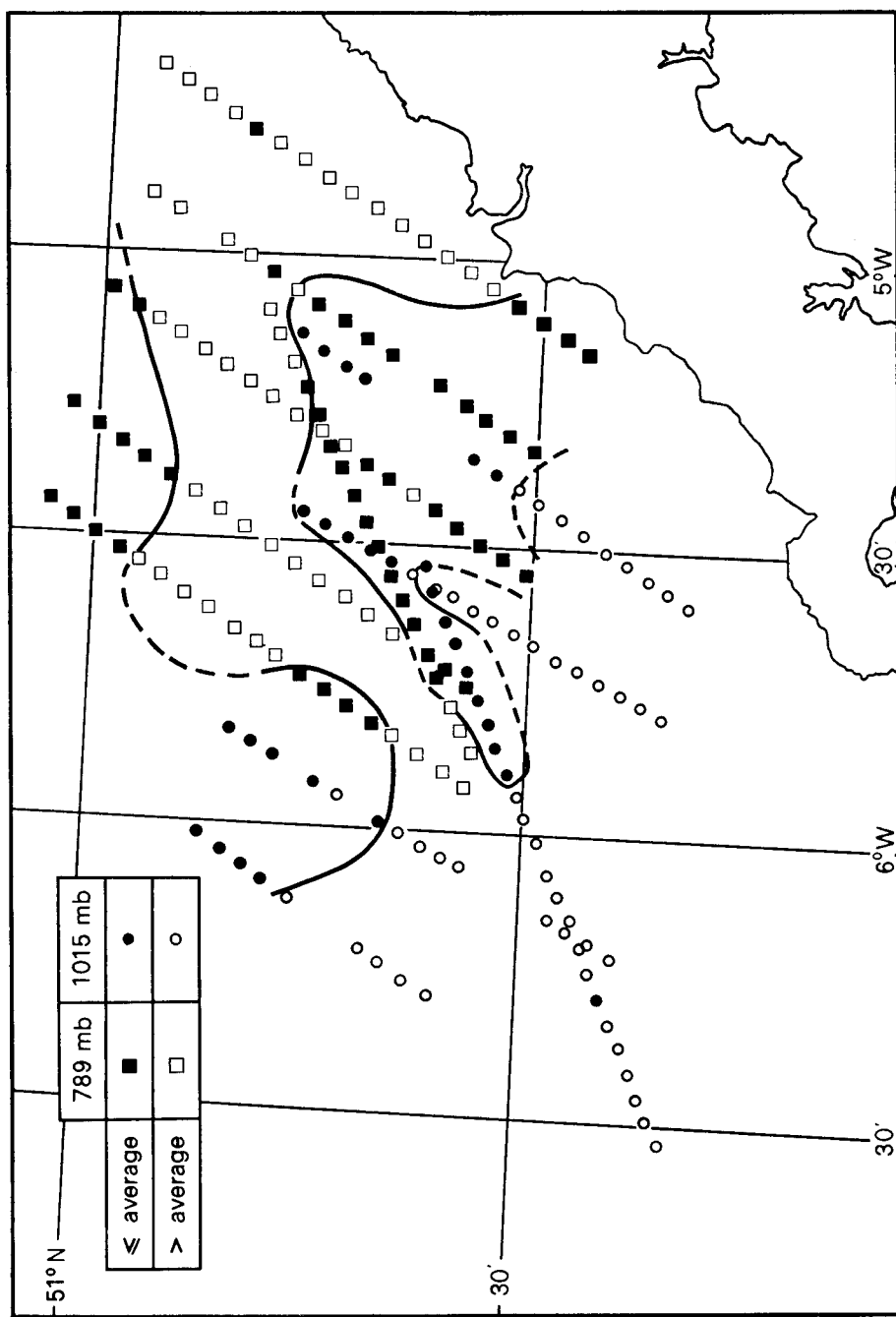


Figure 9. Map showing horizontal variations in turbidity at 1117 GMT on 20 September 1977. Locations are referred to 1117-53 GMT. It is assumed that the air moved at a speed of  $6.5 \text{ m s}^{-1}$  from a direction  $065^\circ$  east of north.



The possibility that the variations in  $\tau_a$  discussed above result from the presence of variable amounts of cirrus cloud has been discounted as no cirrus was observed throughout the flight. Furthermore, the arrival at and the departure from the experimental area were made at high levels (greater than 6 km) at which cirrus cloud is more readily discernible.

### Comparison of low- and high-level spatial variability in turbidity

Until an accurate correction for sensor orientation is determined it will not be possible to consider this topic in detail. However, a preliminary comparison has been made to see if the changes in  $\tau_a$  along the runs at low and high level are associated. In this, the turbidities along each run were expressed as fractions of the average turbidity for that run. Figure 9 shows the result, assuming the whole of the air to move at  $6.5 \text{ m s}^{-1}$  from  $065^\circ$  east of north. All locations in the diagram refer to the position of that part of the air at the start of the experiment. Regions of 'higher than average' and 'lower than average' turbidity do associate reasonably well, supporting the suggestion made above that the low-frequency variations in  $\tau_a$  are largely spatial. Also, these changes appear to persist throughout the experiment and are present both above and below the inversion and, hence, are not solely a feature of the haze layer.

### Conclusions

The installation of an upward-facing pyranometer on the MRF Hercules has been described and sample data presented. These are of high quality and should permit detailed study of both the horizontal and vertical variation in solar irradiance. However, there remains an uncertainty as to the precise orientation of the instrument with respect to the aircraft reference axes. The effect of a marked thermal inversion has been considered and a number of conclusions have been drawn, although it is not known whether or not these are of general applicability as the results from only one flight have been examined:

- (1) The irradiance level increased by about 20 per cent in going from below to above the inversion, as a result of the change in turbidity coefficient.
- (2) Rapid fluctuations were present in the measurements. Above the inversion these have been shown to result from changes in aircraft attitude rather than real changes in irradiance. The data from measurements below the inversion still exhibited rapid fluctuations even after correction for aircraft attitude and are thus a result of changes in turbidity in the haze layer.
- (3) It has been demonstrated that there are longer-term variations during the runs which are spatially correlated not only amongst runs at the same height but also with runs at the other measurement height. So, over a distance of about 60 km, significant variations in turbidity, not connected with the haze layer, have been detected.

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- |   |      |   |
|---|------|---|
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## **An investigation into the variations of grass-minimum depressions**

By M. N. Pickup

(Manchester Weather Centre)

### **Summary**

The mean difference between screen-minimum and grass-minimum temperatures for both clear and cloudy nights was investigated for several stations in England. Little significant variation during cloudy nights was found. With clear skies and light winds the station-to-station variations were significant and it is suggested that they were due not only to differing site surroundings but perhaps also to different characteristics of the surface layers of the ground.

### **Introduction**

Pickup (1976) showed that, during clear nights at West Raynham, mean values of the difference ( $\Delta$ ) between the screen-minimum and the grass-minimum temperatures decreased with increasing wind speed and also that little variation of  $\Delta$  with wind speed occurred during cloudy nights. These results were generally significantly different from those of Craddock and Pritchard (1951) and Saunders (1952) for other locations.

The present investigation was carried out to check whether results similar to those obtained at West Raynham could be found at other stations.

### **Data**

The locations of the stations selected are shown in Figure 1. The selection was limited to stations which had hourly or three-hourly observations on magnetic tape for the period under investigation (January 1961–December 1969).

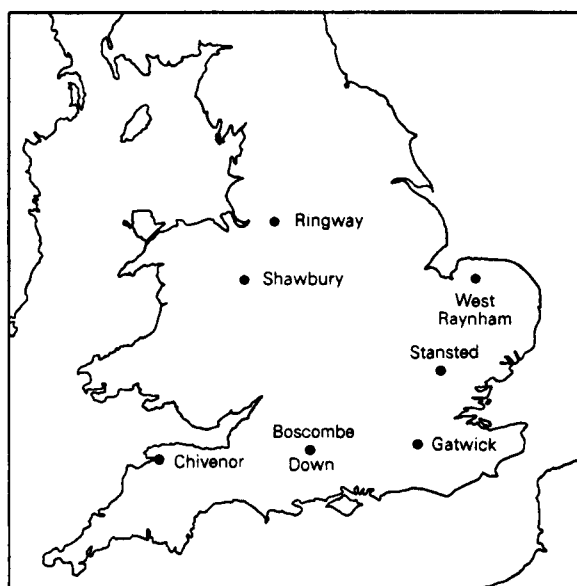


Figure 1. Location of stations referred to in the text.

A computer program was used to extract dates of 'clear' and 'cloudy' nights. A 'clear' night was defined as one with less than 2/8 low cloud, medium cloud, or low and medium cloud combined at each available (hourly or three-hourly) observation between 21 and 09 GMT. Nights with cirriform cloud were included but nights when fog formed were excluded. A 'cloudy' night was defined as one with 8/8 low cloud, medium cloud, or low and medium cloud combined at each available observation between 21 and 09 GMT (excluding nights with precipitation). For each event the average wind speed overnight was computed and the difference ( $\Delta$ ) between the screen-minimum and the grass-minimum temperatures extracted.

The rigid criteria specified to the extraction program produced far fewer events at all the stations (except for clear nights with light winds at some stations) compared with the rather more flexible subjective method used when the original West Raynham observations were classified.

In the original selection of stations Shawbury was not included, but in order to increase the range of the investigation it was added for 'clear' nights only.

## Results

The values of  $\Delta$  for 'cloudy' nights were grouped according to mean surface wind speeds of 1–5, 6–10 and 11–15 knots. The results are shown in Table I. Only Chivenor and Stansted had more than five events in the 11–15 kn category, although even they fell far short of the numbers in the West Raynham sample because there were often brief clear intervals which the objective analysis rejected on otherwise cloudy nights. The totals of cloudy nights for all wind speeds at Boscombe Down (27) and Ringway (28) may seem low for data for a nine-year period, but for these stations cloud amounts and weather were checked for each hour whilst for the other stations they were checked only at three-hourly intervals. The results accorded well with those found at West Raynham, almost all values of  $\Delta$  being less than 1 K.

**Table I.** Depression ( $\Delta$ ) of grass-minimum temperature below screen-minimum temperature on nights with 8/8 low and/or medium cloud, 1961–69

	1–5 knots				Average overnight wind speed				11–15 knots			
			S.E.		6–10 knots		S.E.				S.E.	
	Mean	S.D.	n	of mean	Mean	S.D.	n	of mean	Mean	S.D.	n	of mean
	<i>kelvins</i>				<i>kelvins</i>				<i>kelvins</i>			
West Raynham	0.90	0.49	46	0.07	0.84	0.59	117	0.05	0.70	0.50	80	0.06
Boscombe Down	0.86	0.60	19	0.14	0.53	0.38	8	0.13	—	—	—	—
Chivenor	1.43	1.02	26	0.20	0.83	0.81	25	0.16	1.28	1.56	6	0.64
Gatwick	0.92	0.92	30	0.17	0.88	0.80	17	0.19	—	—	—	—
Ringway	0.62	0.64	18	0.15	0.48	0.18	10	0.06	—	—	—	—
Stansted	0.99	0.94	14	0.25	0.71	0.80	24	0.16	1.07	0.93	8	0.33
S.D. = Standard deviation				n = number of occasions				S.E. = Standard error				

Similar classifications of  $\Delta$  for clear nights are shown in Table II. Although West Raynham, Boscombe Down, Stansted and Shawbury show a gradual decrease of  $\Delta$  with increasing wind speeds the values at Chivenor, Gatwick and Ringway show a maximum  $\Delta$  at the central (6–10 kn) range of wind speeds. This maximum is only statistically significant, however, (at the 5 per cent level) at Gatwick.

**Table II.** Depression ( $\Delta$ ) of grass-minimum temperature below screen-minimum temperature on nights with less than 2/8 low and/or medium cloud, 1961–69

	Average overnight wind speed											
	1–5 knots				6–10 knots				11–15 knots			
	Mean kelvins	S.D.	<i>n</i>	S.E. of mean	Mean kelvins	S.D.	<i>n</i>	S.E. of mean	Mean kelvins	S.D.	<i>n</i>	S.E. of mean
West Raynham	5.34	1.03	118	0.09	3.71	1.17	327	0.06	2.08	0.51	135	0.04
Boscombe Down	4.52	1.39	95	0.14	3.11	1.45	65	0.18	2.58	1.36	29	0.25
Chivenor	3.33	1.34	104	0.13	3.51	1.42	65	0.18	3.10	1.26	17	0.31
Gatwick	3.29	1.09	137	0.09	4.04	1.21	39	0.19	2.87	0.51	7	0.19
Ringway	2.94	1.16	82	0.13	2.98	1.16	49	0.17	2.52	0.95	5	0.42
Stansted	4.86	1.51	96	0.15	3.34	1.34	102	0.13	2.33	0.77	17	0.19
Shawbury	5.12	1.04	105	0.10	4.68	1.22	32	0.22	3.47	0.75	8	0.27

S.D. = Standard deviation

*n* = number of occasions

S.E. = Standard error

## Discussion

Student's *t*-tests were made on the differences between the means for the various stations on cloudy nights. The results are shown in Table III. Against each station are listed those other stations which showed no significant difference between the means (probability  $P = 5$  per cent) for each wind-speed category. In each of the 1–5 kn and 6–10 kn categories, 12 out of 15 were not significantly different. None of the three samples with winds 11–15 kn were significantly different. Thus 27 out of a total of 33 tests showed no significant differences between the means. Similarly *t*-tests showed that few of the differences between these average values for different speed ranges for the same stations are statistically significant at the 5 per cent level. It seems therefore to be appropriate to suggest that a value of  $\Delta$  of 0.9 K (or 1 K if working in whole degrees) would be suitable for all stations during cloudy nights irrespective of wind speed. However, if the differences found here are indeed representative true differences, then in view of the scatter in the results and the numbers of events involved, it should be realized that the odds in favour of failing to find statistically significant differences in these trials are nearly all well above evens—both for differences between stations for the same speed range and between speed ranges for the same stations. It has also to be remembered that Shawbury—the station where the combined effects of soil density, specific heat capacity and thermal conductivity differ most from those of the other stations (see below)—was not included in this investigation for cloudy nights.

Similarly, *t*-tests were carried out on the data for clear nights. The results are shown in Table IV. In the 1–5 kn category only 4 out of a total of 21 tests were not significantly different. This increased to 7 out of 21 in the 6–10 kn range and to 15 out of 21 with winds of 11–15 kn. These results indicate that in relation to the scatter of the data and the numbers of events involved in the analysis the effects of site differences become less important as the wind speed increases and that the inter-station differences apparent when winds are above 10 kn could, for the most part, arise readily enough by chance. Some comparisons involving West Raynham—where the number of events is much larger than elsewhere—and Shawbury—where mean values of  $\Delta$  are high—provide notable exceptions. Thus  $\Delta$  values for one site are likely to provide poor estimates of  $\Delta$  for other locations in situations with light winds or clear skies. On some airfields the enclosure is surrounded by 'pans' (extensive areas of level concrete), taxi-ways and buildings, which may mean that with light winds cooling is reduced by conduction from the surroundings. Another factor, however, may be the variation in soil types on the sites.

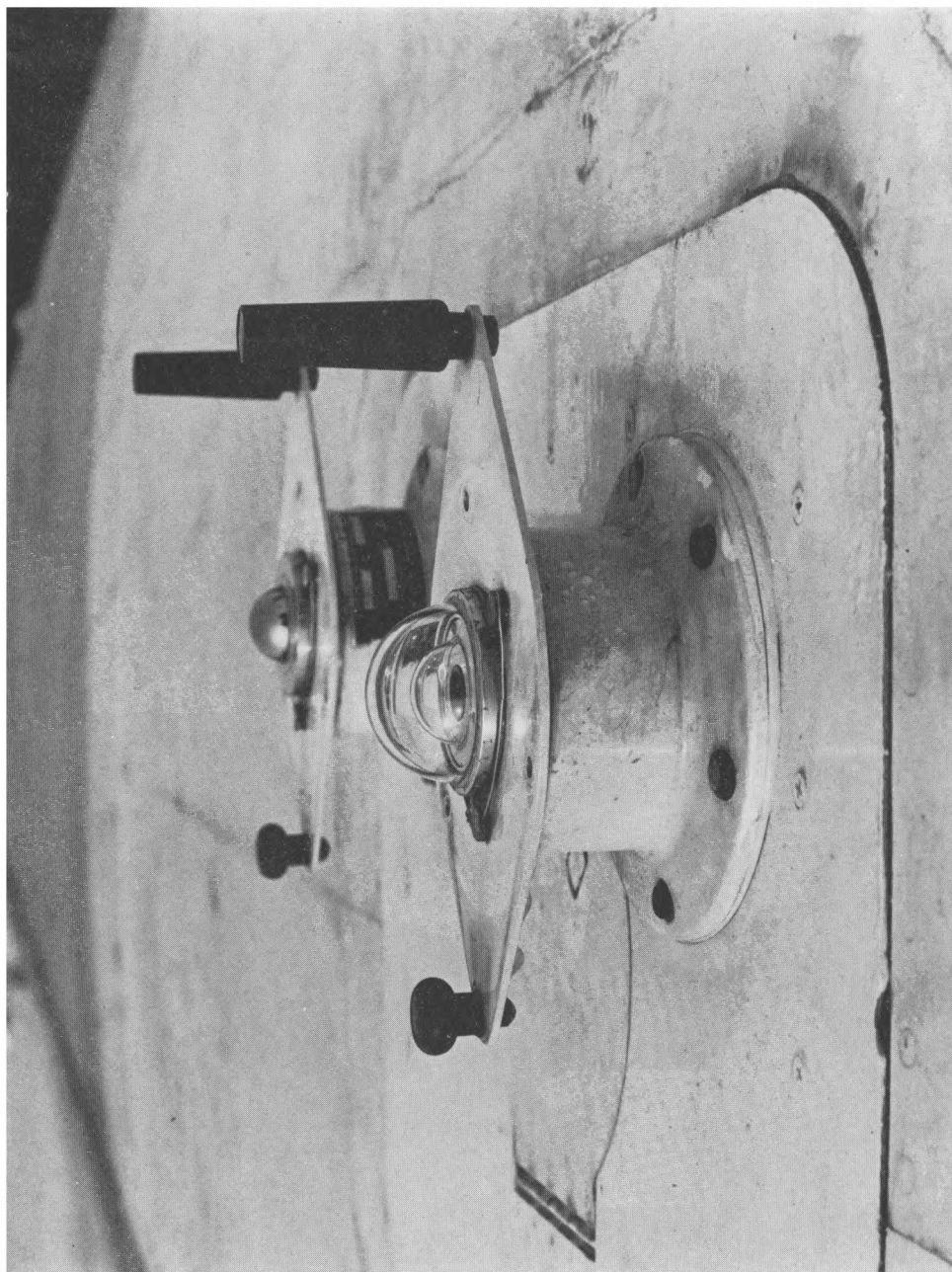


Plate I. An Eppley pyranometer installed on a Hercules aircraft of the Meteorological Research Flight. (See page 217.)



Plate II. General view of the Museum of Meteorological Instruments at Meteorological Office Headquarters, Bracknell. (See page 24b.)

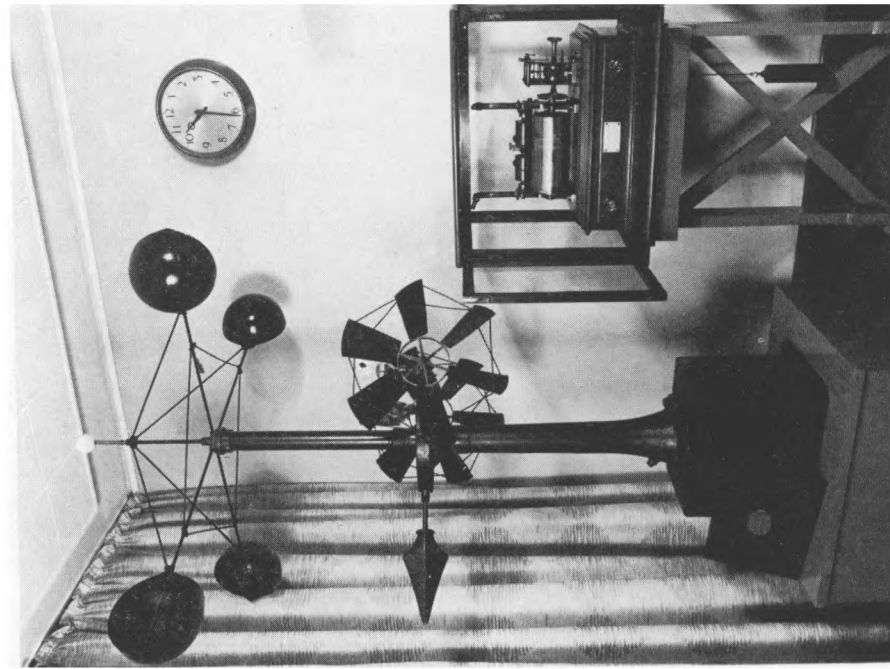


Plate III. Beckley-Robinson anemograph.

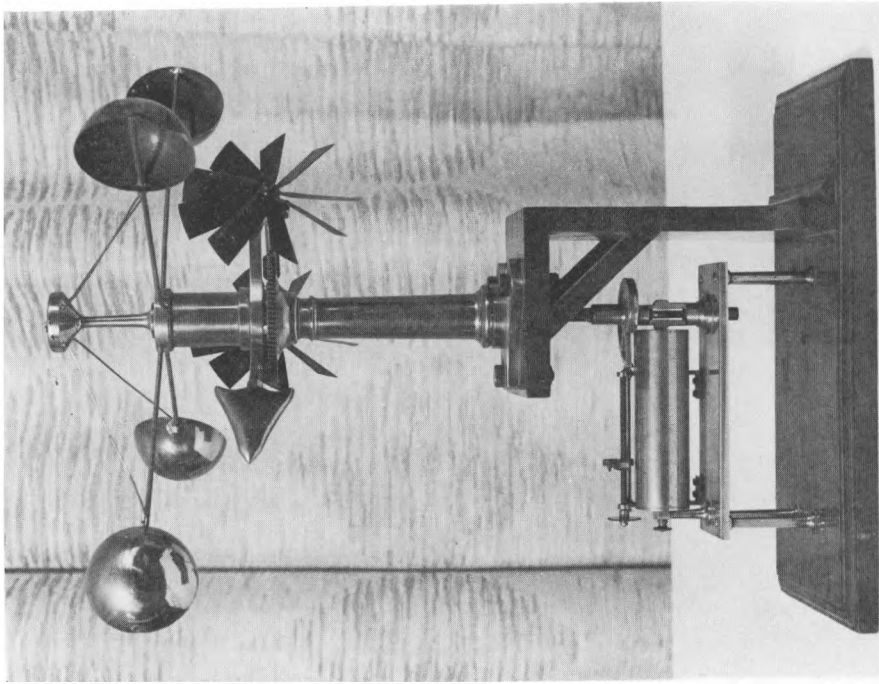


Plate IV. Prototype model wind vane constructed by Robert Beckley.



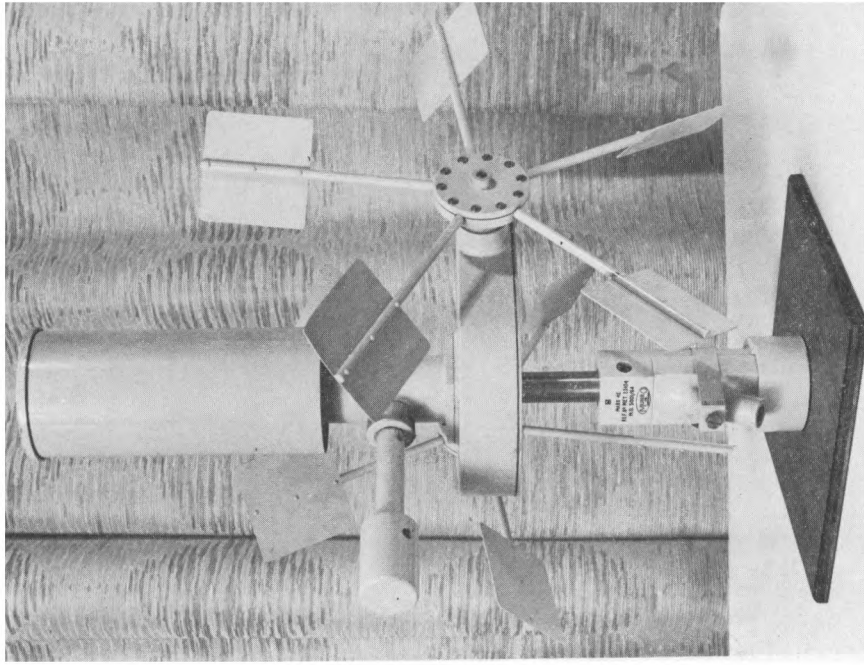


Plate V. Modern Mk 4E wind vane.

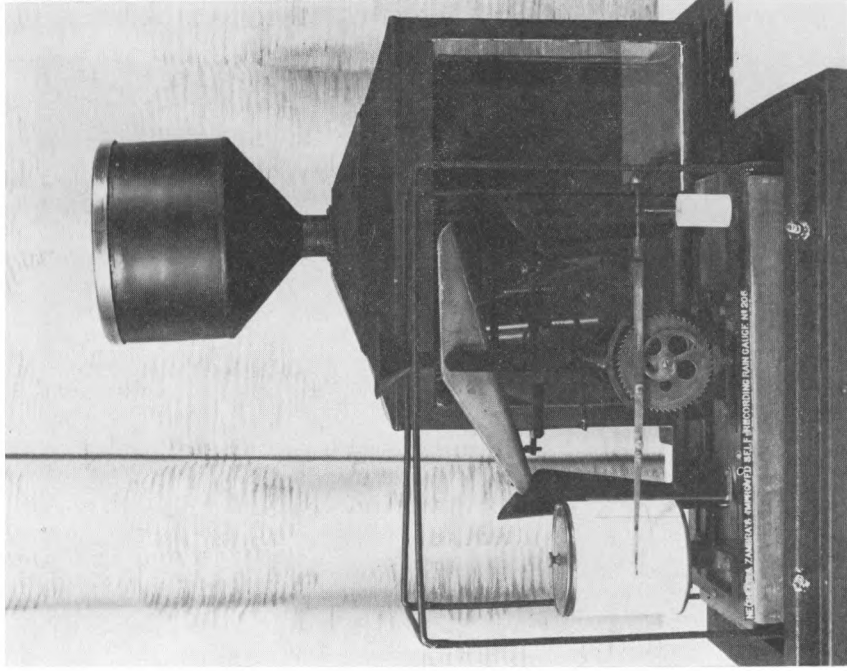


Plate VI. Negretti & Zambra improved self-recording rain-gauge (c. 1890).



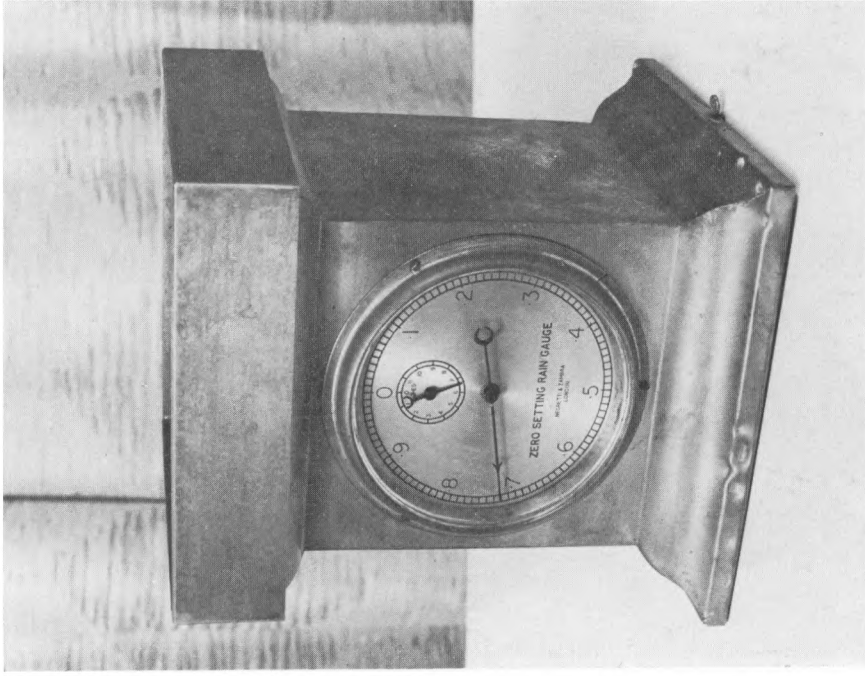


Plate VII. Negretti & Zambra dial rain-gauge.

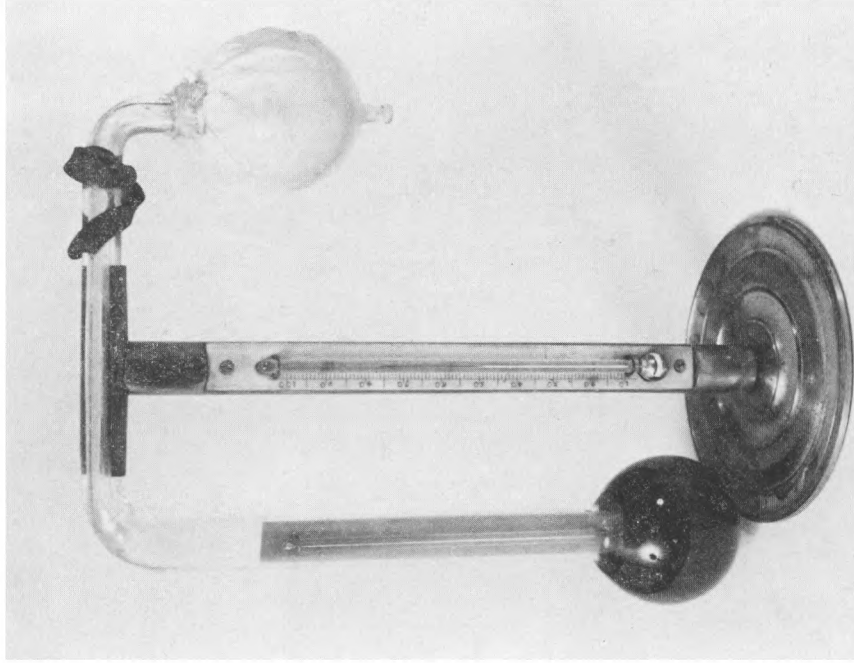


Plate VIII. Daniell's dew-point hygrometer (1860).



Plate IX. Kater's hygrometer (c. 1812).

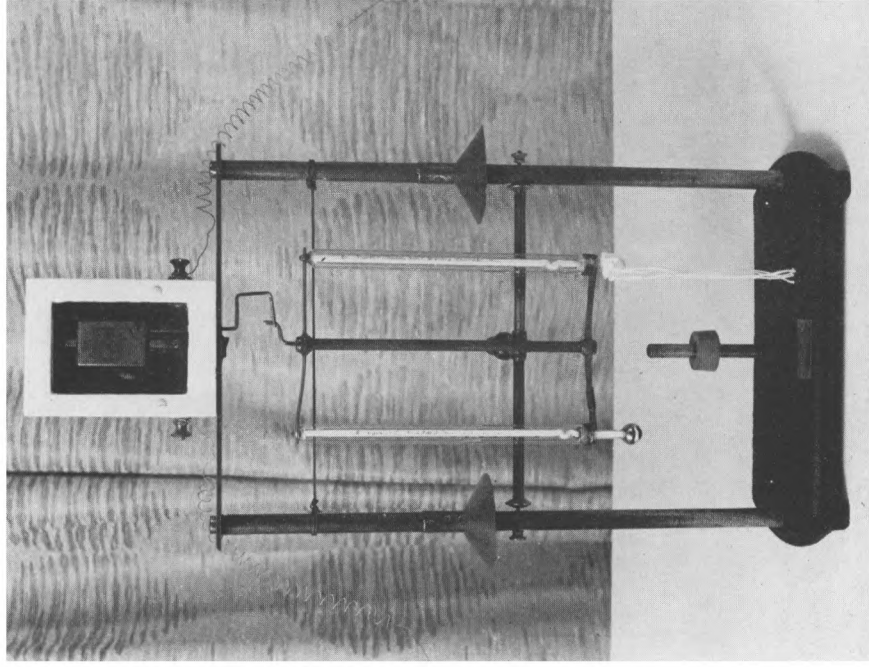


Plate X. Negretti & Zambra recording thermometer (c. 1898).

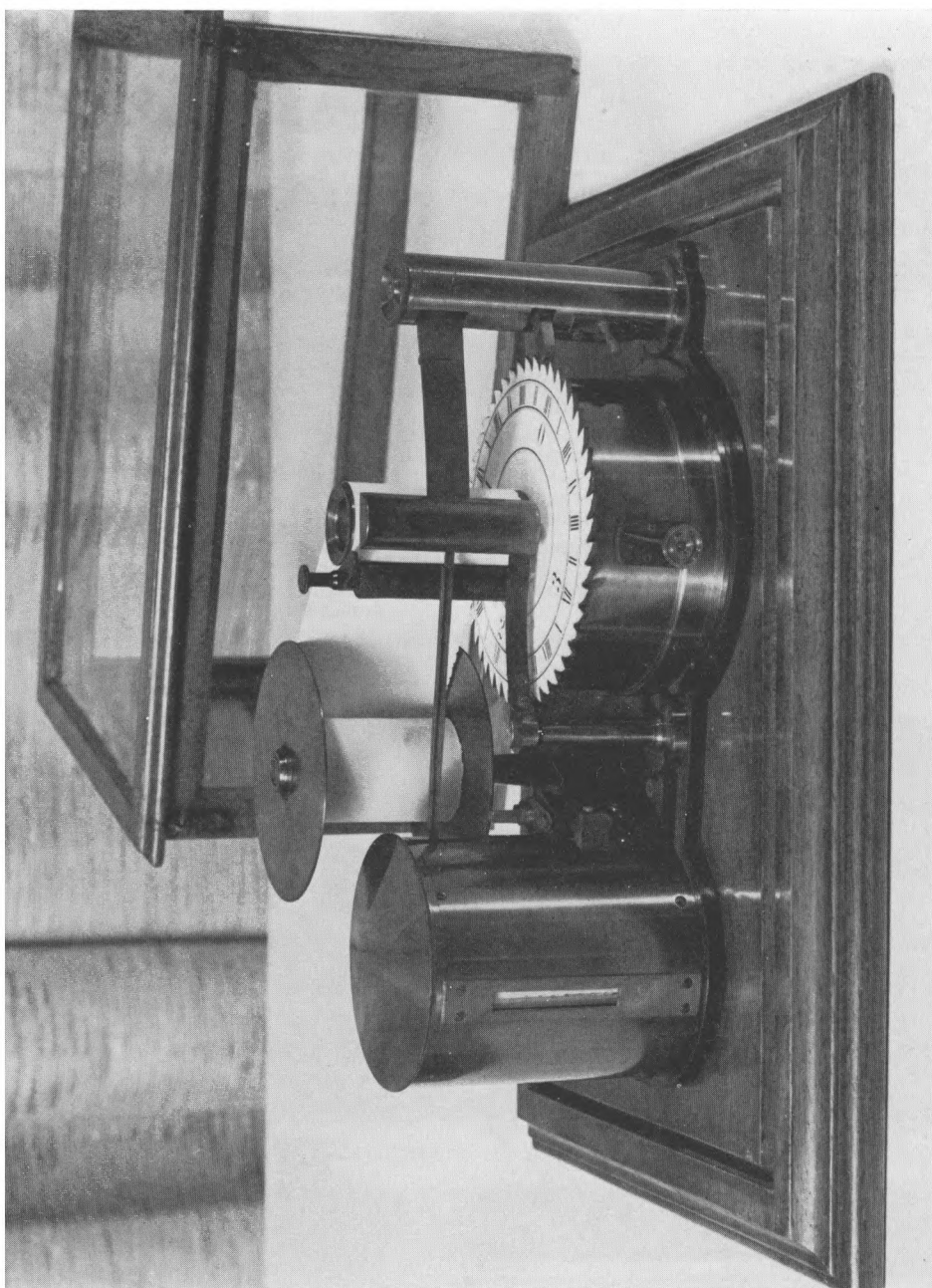


Plate XI. Goldschmid barograph (1880).

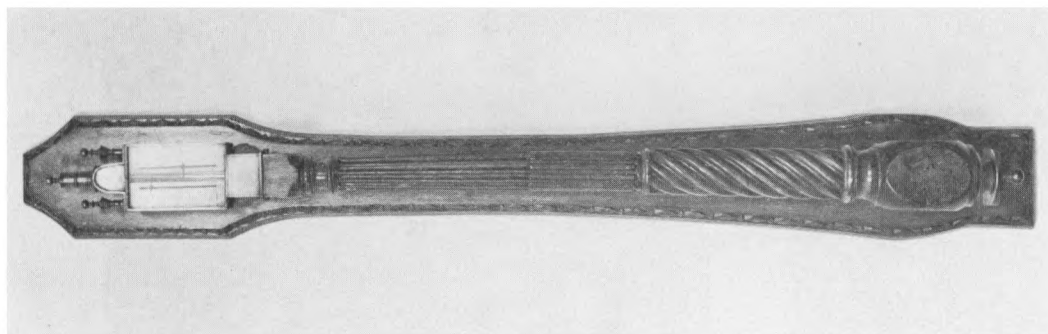


Plate XIII. Mercury barometer by Dan Quare (c. 1695).

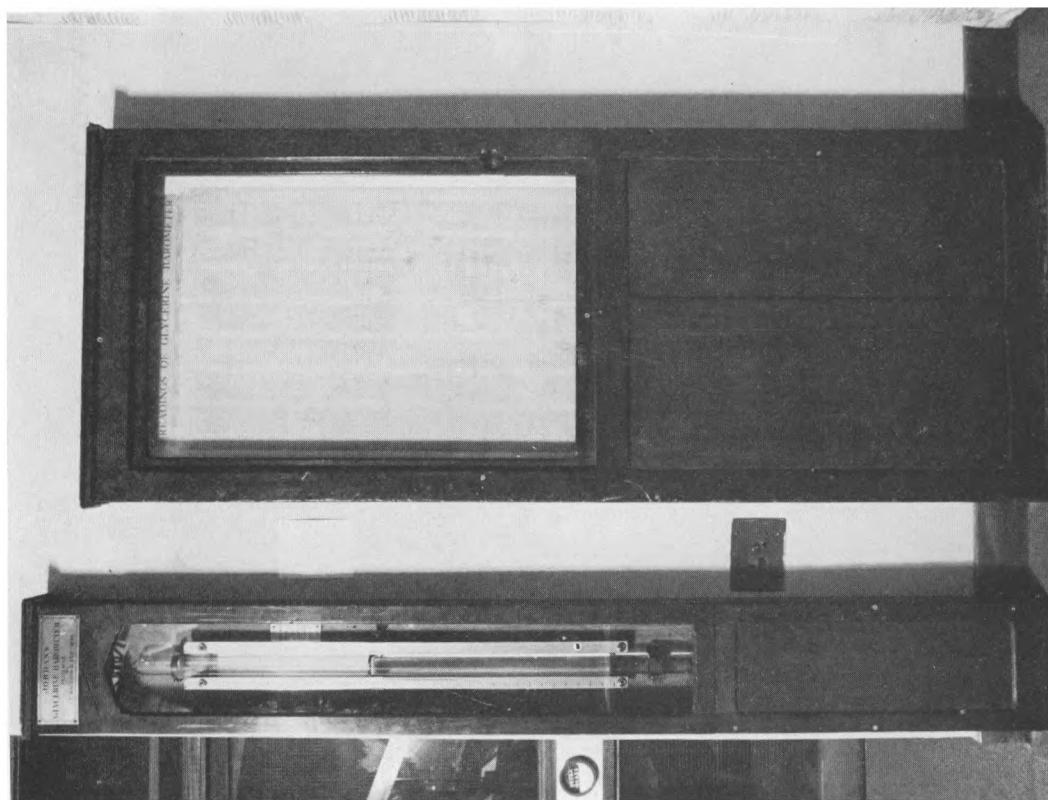


Plate XII. Top part of Jordan glycerine barometer.

**Table III.** Results of Student's *t*-tests to compare means of  $\Delta$  against control stations during cloudy nights. Stations listed were not significantly different ( $P = 5$  per cent)

Control Station	Average wind speed		
	1-5 knots	6-10 knots	11-15 knots
West Raynham	Boscombe Down Gatwick Ringway Stansted	Chivenor Gatwick Stansted	Chivenor Stansted
Boscombe Down	West Raynham Gatwick Ringway Stansted	Chivenor Gatwick Ringway Stansted	*
Chivenor	Gatwick Stansted	West Raynham Boscombe Down Gatwick Stansted	West Raynham Stansted
Gatwick	West Raynham Boscombe Down Chivenor Ringway Stansted	West Raynham Boscombe Down Chivenor Ringway Stansted	*
Ringway	West Raynham Boscombe Down Gatwick Stansted	Boscombe Down Gatwick Stansted	*
Stansted	West Raynham Boscombe Down Chivenor Gatwick Ringway	West Raynham Boscombe Down Chivenor Gatwick Ringway	West Raynham Chivenor

\* Boscombe Down, Gatwick and Ringway had no data with winds 11-15 knots.

### Effect of soil type

Brunt (1939) showed that, in general, turbulence is a far more effective agent than radiation in transferring heat vertically through the atmosphere. But he also showed that at very small heights above the ground when turbulence is unable to develop effectively, the transfer of heat there is mainly by radiation. With simplifying assumptions, Brunt calculated that the nocturnal fall of temperature at the ground was proportional to the square root of the time from sunset and inversely proportional to  $\rho ca^{\frac{1}{2}}$ , where  $\rho$  is density,  $c$  specific heat capacity and  $a$  the thermal diffusivity of the ground. The fall of surface temperature over sandy soil, where for dry sand  $(\rho ca^{\frac{1}{2}})^{-1} \approx 71$ —a value which decreases rapidly as the water content is increased—should be greater than over concrete, where for concrete used in dams  $(\rho ca^{\frac{1}{2}})^{-1} \approx 18$  (using values in appropriate units from the *Smithsonian meteorological tables*, 1958 edition, Table 122).

Zdunkowski and Trask (1971) applied a radiative-conductive numerical model to the simulation of nocturnal temperature change over four soil types for a night with clear skies and a constant gradient wind of 10 m/s (20 kn) (which led to 10 m winds of about 10 kn), and assuming no deposition of dew. From their graphs of cooling from sunset for the earth's surface (simulated roughness appropriate to short grass) and for 180 cm above the surface, values of the depression ( $D$ ) of surface temperature

**Table IV.** Results of Student's *t*-tests to compare means of  $\Delta$  against control stations during clear nights. Stations listed were not significantly different ( $P = 5$  per cent)

		Average wind speed	
		1-5 knots	6-10 knots      11-15 knots
Control station			
West Raynham	Shawbury	Chivenor Gatwick	Boscombe Down Ringway Stansted
Boscombe Down	Stansted	Chivenor Ringway Stansted	West Raynham Chivenor Gatwick Ringway Stansted
Chivenor	Gatwick	West Raynham Boscombe Down Stansted	Boscombe Down Gatwick Ringway Shawbury
Gatwick	Chivenor	West Raynham	Boscombe Down Chivenor Ringway Stansted Shawbury
Ringway		Boscombe Down Stansted	West Raynham Boscombe Down Chivenor Gatwick Stansted Shawbury
Stansted	Boscombe Down Shawbury	Boscombe Down Chivenor Ringway	West Raynham Boscombe Down Gatwick Ringway Chivenor Gatwick Ringway
Shawbury	West Raynham Stansted		

below the temperature at 180 cm eight hours after sunset were estimated for their four soil types. These are shown in Table V together with associated values of  $(\rho c a^{\frac{1}{2}})^{-1}$  for the soils they considered.

Details of the soil at the stations under investigation here were obtained from *Aerodrome weather diagrams and characteristics* and are shown in Table VI. They are unfortunately too vague to use for estimating values of  $(\rho c a^{\frac{1}{2}})^{-1}$ , and enclosures are not necessarily representative of the surrounding soil type—especially at Ringway. A comparison of the theoretical  $D$  with the observed  $\Delta$  is difficult, especially since water content has a considerable effect. We can, however, try to estimate the range of

**Table V.** Values of  $(\rho c a^{\frac{1}{2}})^{-1}$  and the depression of surface temperature below that at the 180 cm level for various soil types

Soil type	$(\rho c a^{\frac{1}{2}})^{-1}$ [cm <sup>2</sup> K s <sup>1/2</sup> cal <sup>-1</sup> ]	$D$ after 8 hours kelvins
Quartz sand, medium fine dry	71	5.4
Sandy clay, 15 per cent moisture	28	3.2
Humus	25	2.9
Rocky soil	13	2.2

Values of  $D$  are derived from Figures 5 and 6 of Zdunkowski and Trask (1971) and represent the depression of surface temperature below that at a height of 180 cm 8 hours after sunset on a simulated clear night.

**Table VI.** *Details of the soil type at each station*

Station	Height above sea level metres	Soil type
West Raynham	78	Loamy soil covering clay with a chalk base
Boscombe Down	124	Chalk downland
Chivenor	8	Alluvial clay—not well drained
Gatwick	59	Clay subsoil—holds water near the surface
Ringway	78	Sand and clay loam
Stansted	106	Subsoil, heavy clay—poor drainage
Shawbury	76	Sand and gravel loam

$(\rho c a^{\frac{1}{2}})^{-1}$  among stations assuming the theory to be correct. From Table II the range of  $\Delta$  among stations for a 10 kn wind on clear nights, obtained by extrapolating between the ranges, is approximately 2.8 to 4.2 K. Assuming a 12-hour night and a  $t^{\frac{1}{2}}$  cooling rate, this is equivalent to 2.3 to 3.4 K for an 8-hour cooling period, giving  $(\rho c a^{\frac{1}{2}})^{-1}$  in the range 15 to 35 units. This range agrees fairly well with the values in Table V and suggests that better estimates of  $(\rho c a^{\frac{1}{2}})^{-1}$  for individual sites might help in predicting  $\Delta$ .

### Some large observed values of $\Delta$

With clear skies and light winds, heat transfer is mainly by radiation and large differences between screen and grass temperatures frequently occur shortly after sunset with a rapid fall of ground temperature. Several occasions when this occurred at West Raynham in 1973 and 1974 are listed in Table VII.

**Table VII.** *Observed values of  $\Delta$  at West Raynham*

Date	Sunset (GMT)	Time (GMT)	Air min. degrees Celsius	Grass min. Celsius	$\Delta$ kelvins
1.2.73	1645	18–21	1.3	—4.5	5.8
26.3.73	1823	19	7.4	—3.0	10.4
		18–21	3.3	—3.1	6.4
25.4.73	1914	18–21	5.0	—3.5	8.5
1.5.73	1928	22	6.9	—5.6	12.5
		23	5.8	—4.8	10.6
		21–09	2.0	—6.2	8.2
1.10.73	1738	18–21	6.9	1.5	5.4
23.10.73	1648	18–21	5.2	—1.0	6.2
20.11.73	1558	20	0.5	—5.9	6.4
		18–21	—0.1	—6.0	5.9
13.3.74	1759	18–21	2.8	—3.5	6.3
14.3.74	1802	18–21	0.8	—6.4	7.2
20.3.74	1812	18–21	5.1	—2.5	7.6
27.8.74	1901	19–21	10.5	3.5	7.0

When a range of times is given the temperatures recorded are each the minimum for that period. It is evident that on each of the three nights when temperatures for more than one period were recorded, although both air temperatures and the grass temperatures continued to fall, the values of  $\Delta$  decreased as time passed. This implies that  $\Delta$  based upon night minima will be less than values for  $\Delta$  experienced early in the night.

Before January 1978 grass minimum temperatures were officially recorded between 21 and 09 GMT. During trials at Porton Down in April 1977 a continuous trace of air temperature and grass-minimum depression was obtained for several nights, between 18 and 06 GMT, using platinum resistance thermometers. The results for periods ending at 05 GMT for two clear nights with light winds, 4/5 April and

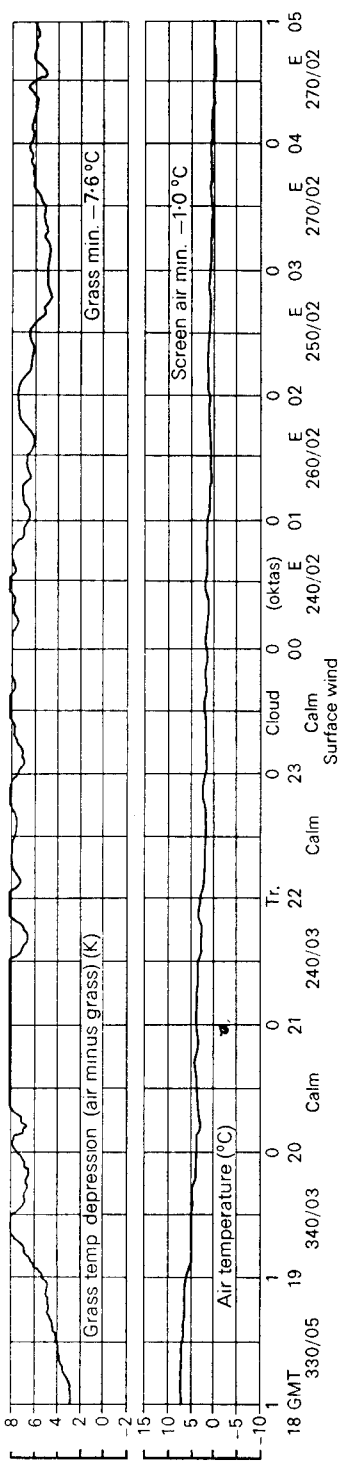


Figure 2. Diagrammatic representation of air temperature and grass-temperature depression recorded at Porton Down between 18 GMT on 4 April 1977 and 05 GMT on 5 April 1977 (sunset 1844 GMT).

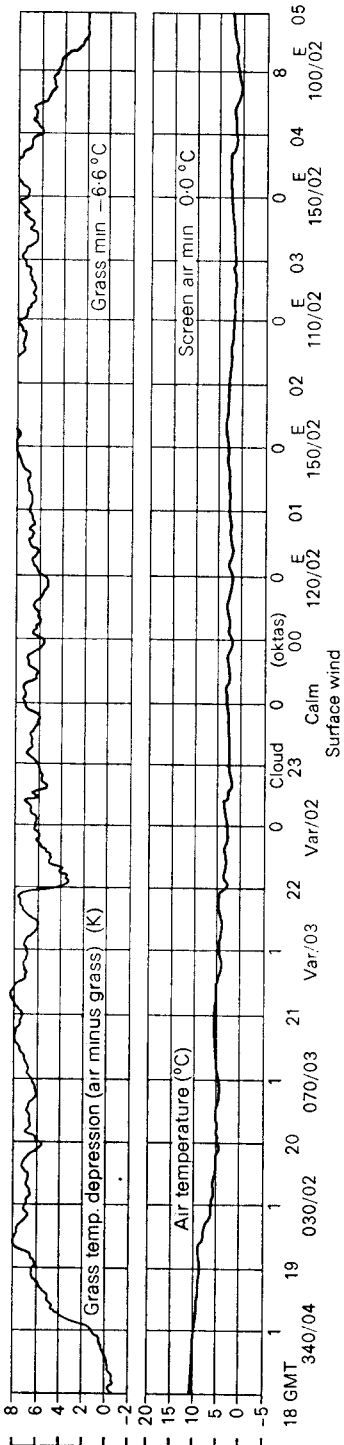


Figure 3. Diagrammatic representation of air temperature and grass-temperature depression recorded at Porton Down between 18 GMT on 15 April 1977 and 05 GMT on 16 April 1977 (sunset 1902 GMT).



15/16 April, are shown in Figures 2 and 3. On both occasions the value of  $\Delta$  overnight was 6.6 K although depressions of 8 K occurred within an hour of sunset. In fact, on the 15th a depression of 6 K was recorded at sunset (1902 GMT). This is an important factor, especially during the winter months, when considering the possibility of ground frost.

### Conclusions

For cloudy nights a value of screen-minimum minus grass-minimum temperature ( $\Delta$ ) equal to about 0.9 K applies to the different sites considered, irrespective of wind speed overnight.

For clear nights the value of  $\Delta$  usually decreases with increasing wind speed but variations occur, especially with light winds, probably owing both to local site characteristics and to the effects of the various soil types.

With clear skies and light winds large depressions of grass temperature below the screen temperature frequently occur shortly after sunset. These depressions are often several degrees larger than values of  $\Delta$  based upon night minimum temperatures, and are more likely to occur when the soil is dry, since  $(\rho c a)^{-1}$  decreases with increasing water content. Under similar conditions, large differences can also be expected between grass-minimum and concrete-minimum temperatures.

### Acknowledgements

The author would like to thank Mr C. L. Hawson and other members of the staff of the Outstations Investigations Section of the Special Investigations Branch of the Meteorological Office for help in the preparation of this paper.

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- |                                    |      |   |
|------------------------------------|------|---|
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551.501.4:551.571

## Computation of vapour pressure, dew-point and relative humidity from dry- and wet-bulb temperatures

By G. P. Sargent

(Meteorological Office, Bracknell)

### Summary

A current project in the Operational Instrumentation Branch (Met O 16) of the Meteorological Office, Bracknell, required the collation of procedures for computing atmospheric moisture variables from readings of dry- and wet-bulb temperatures. It was thought that such a collation would prove a useful addition to the literature. This paper, therefore, describes some of the procedures that are available.

### Introduction

The dry- and wet-bulb psychrometer is one of the commonest instruments used for obtaining information about the moisture content of the atmosphere. It is a relatively simple instrument and, provided that precautions are taken to maintain the wet bulb properly, it is capable of giving good results.

One of the drawbacks is that the computations of vapour pressure, dew-point and relative humidity are complex and not easily achieved by the use of small electronic calculators or microprocessors.

This paper sets out various equations which may be used for these computations, with an indication of the accuracy of the computation relative to the Goff-Gratch formula.

The following details concerning the notation used in these equations should be noted:  $\exp_{10} x \equiv 10^x$ ; 'lg' implies logarithm to base 10; 'ln' implies logarithm to base e.

### Equations for saturation vapour pressure

*Goff-Gratch.* The Goff-Gratch equations (Goff and Gratch, 1945) are the bases of the tables of saturation vapour pressure published in the *Smithsonian meteorological tables* and of the Meteorological Office humidity slide-rule.

For saturation vapour pressure with respect to a plane surface of pure water,

$$\begin{aligned} \lg e_w = & -7.90298(T_s/T - 1) + 5.02808 \lg(T_s/T) - \\ & -1.3816 \times 10^{-7} [\{\exp_{10} 11.344(1 - T/T_s)\} - 1] + \\ & + 8.1328 \times 10^{-8} [\{\exp_{10} -3.49149(T_s/T - 1)\} - 1] + \lg e_{ws}, \quad \dots \quad (1) \end{aligned}$$

where  $e_w$  = saturation vapour pressure in millibars with respect to a plane surface of pure water at temperature  $T$ ,

$T$  = temperature in kelvins,

$T_s$  = temperature at the steam-point (= 373.16 K), and

$e_{ws}$  = saturation vapour pressure in millibars at the steam-point temperature (= 1013.246 mb).

For saturation vapour pressure with respect to a plane surface of pure ice,

$$\begin{aligned} \lg e_i = & -9.0718(T_0/T - 1) - 3.56654 \lg(T_0/T) + \\ & + 0.876793(1 - T/T_0) + \lg e_{i0}, \quad \dots \quad (2) \end{aligned}$$

where  $e_i$  = saturation vapour pressure in millibars with respect to a plane surface of pure ice,

$T$  = temperature in kelvins,

$T_0$  = temperature at the ice-point (= 273.16 K), and

$e_{i0}$  = saturation vapour pressure in millibars at the ice-point temperature (= 6.1071 mb).

These equations are based on theoretical considerations and experimental data. Equation (1) is valid for temperatures between 0 °C and 100 °C and is accepted for temperatures between 0 °C and — 50 °C until further research indicates it to be in error. It should be noted that below 0 °C equation (1) relates to supercooled water. Equation (2) is valid for temperatures between 0 °C and — 100 °C.

The Goff-Gratch equations were formulated using the old Kelvin scale of temperature and the old definition of a standard atmosphere. Goff (1965) later transposed equations (1) and (2) to take into account the new Kelvin scale of temperature and the current definition of a standard atmosphere:

$$\lg(e_w/e_{ws}') = 10.79586(1 - T_{01}/T) - 5.02808 \lg(T/T_{01}) + \\ + 1.50474 \times 10^{-4} [1 - \{\exp_{10} - 8.29692(T/T_{01} - 1)\}] + \\ + 0.42873 \times 10^{-3} [\{\exp_{10} 4.76955(1 - T_{01}/T)\} - 1] - \\ - 2.2195983, \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

$$\lg(e_i/e_{ws}') = -9.096936(T_{01}/T - 1) - 3.56654 \lg(T_{01}/T) + \\ + 0.876817(1 - T/T_{01}) - 2.2195983, \quad \dots \quad \dots \quad \dots \quad \dots \quad (4)$$

where  $e_w$  and  $e_i$  are as in equations (1) and (2),

$e_{ws}'$  = saturation vapour pressure in millibars at the steam-point temperature (new scale) (= 1013.25 mb),

$T$  = temperature in kelvins (new scale), and

$T_{01}$  = temperature of the triple point of water in kelvins (new scale) (= 273.16 K).

It should be noted that equations (3) and (4) give saturation vapour pressures differing from those given by equations (1) and (2) by amounts corresponding to temperature differences of less than 0.005 °C throughout the range — 100 °C to + 100 °C. The differences can thus be ignored for practical psychrometry.

The Goff-Gratch equations are generally accepted as giving the closest approximations to the true values of saturation vapour pressure, but they do not lend themselves to easy manipulation, especially if it is desired to find the temperature corresponding to a given value of saturation vapour pressure.

Various workers have approximated the Goff-Gratch relationship with expressions which are more amenable to automatic computation.

*Hooper.* During the development of the Meteorological Office Mk 3 radiosonde system, Hooper (unpublished working paper) developed a sixth-order polynomial to compute saturation vapour pressures with respect to a plane water surface. The coefficients were determined by Clenshaw's method of orthogonal polynomials (Clenshaw, 1960). It is claimed to be valid between — 40 °C and + 50 °C.

$$e_w = \exp(L_0 + L_1T + L_2T^2 + L_3T^3 + L_4T^4 + L_5T^5 + L_6T^6), \dots \quad (5)$$

where  $e_w$  = saturation vapour pressure in millibars with respect to a plane surface of pure water

$T$  = temperature in degrees Celsius

$L_0$  = 1.809544921

$L_1$  =  $7.266467428 \times 10^{-2}$

$L_2$  =  $-2.996322270 \times 10^{-4}$

$L_3$  =  $1.155167035 \times 10^{-6}$

$L_4$  =  $-4.564108094 \times 10^{-9}$

$L_5$  =  $2.687871955 \times 10^{-11}$

$L_6$  =  $-1.603993935 \times 10^{-13}$ .

Over the quoted range this has a maximum departure from Goff-Gratch at  $+50^\circ\text{C}$  of  $-8.63 \times 10^{-3}$  mb.

Examination of the coefficients for Hooper's polynomial reveals that they can be adjusted to give a closer approximation to the Goff-Gratch results. Thus:

$$\begin{aligned} L_0 &= 1.809567918 \\ L_1 &= 7.266296315 \times 10^{-2} \\ L_2 &= -2.996403370 \times 10^{-4} \\ L_3 &= 1.160464233 \times 10^{-6} \\ L_4 &= -4.606513971 \times 10^{-9} \\ L_5 &= 2.315159066 \times 10^{-11} \\ L_6 &= -1.103513358 \times 10^{-13}. \end{aligned}$$

These coefficients give a maximum departure from Goff-Gratch of  $-1.88 \times 10^{-4}$  mb at  $+50^\circ\text{C}$  over the range  $-40^\circ\text{C}$  to  $+50^\circ\text{C}$ .

*Richards.* Richards (1971) presented an equation to approximate the Goff-Gratch equation, claiming sufficient accuracy for most purposes. The coefficients have been adjusted to minimize the errors, using Powell's method (Powell, 1965).

$$e_w = e_{ws}' \exp(13.3185t - 1.9760t^2 - 0.6445t^3 - 0.1299t^4), \quad \dots \quad (6)$$

where  $t = (1 - T_s/T)$ ,

$e_w$  = saturation vapour pressure in millibars with respect to a plane surface of pure water,

$e_{ws}'$  = saturation vapour pressure in millibars at the steam-point temperature (new scale) (= 1013.25 mb),

$T$  = temperature in kelvins (new scale), and

$T_s$  = temperature at the steam-point (= 373.15 K).

Richards claims a normalized discrepancy of less than 0.001 between his formula and Goff-Gratch between  $-50^\circ\text{C}$  and  $140^\circ\text{C}$ . In the range  $-40^\circ\text{C}$  to  $+50^\circ\text{C}$  the maximum departure from Goff-Gratch is  $1.611 \times 10^{-2}$  mb at  $+50^\circ\text{C}$ .

*Lowe.* Lowe (1977) presented a series of polynomials to approximate saturation vapour pressures. To achieve a similar degree of accuracy to Richards's method a sixth-degree polynomial is required. The coefficients were determined by means of a Chebyshev fitting procedure. For a temperature range  $-50^\circ\text{C}$  to  $+50^\circ\text{C}$ :

$$e_w = C_0 + C_1T + C_2T^2 + C_3T^3 + C_4T^4 + C_5T^5 + C_6T^6, \quad \dots \quad (7)$$

where  $e_w$  = saturation vapour pressure in millibars with respect to a plane surface of pure water

$T$  = temperature in degrees Celsius

$$C_0 = 6.107799961$$

$$C_1 = 4.436518521 \times 10^{-1}$$

$$C_2 = 1.428945805 \times 10^{-2}$$

$$C_3 = 2.650648471 \times 10^{-4}$$

$$C_4 = 3.031240396 \times 10^{-6}$$

$$C_5 = 2.034080948 \times 10^{-8}$$

$$C_6 = 6.136820929 \times 10^{-11}.$$

The maximum departure from Goff-Gratch over the range  $-40^\circ\text{C}$  to  $+50^\circ\text{C}$  is  $-1.264 \times 10^{-2}$  mb and occurs at  $+50^\circ\text{C}$ .

For an ice surface from  $-50^{\circ}\text{C}$  to  $0^{\circ}\text{C}$ :

$$\begin{aligned}C_0 &= 6.109177956 \\C_1 &= 5.03469897 \times 10^{-1} \\C_2 &= 1.886013408 \times 10^{-2} \\C_3 &= 4.176223716 \times 10^{-4} \\C_4 &= 5.824720280 \times 10^{-6} \\C_5 &= 4.838803174 \times 10^{-8} \\C_6 &= 1.838826904 \times 10^{-10}.\end{aligned}$$

These coefficients will give  $e_i$ , the saturation vapour pressure in millibars with respect to a plane surface of pure ice.

### Vapour pressure

To compute the ambient vapour pressure of the air, the psychrometric equation is used:

$$e = e_w' - AP(T - T_w), \quad \dots \quad (8)$$

where  $e$  = ambient vapour pressure in millibars,

$e_w'$  = saturation vapour pressure in millibars with reference to water at the wet-bulb temperature,

$A$  = the psychrometric coefficient,

$P$  = atmospheric pressure in millibars,

$T$  = dry-bulb temperature in degrees Celsius,

$T_w$  = wet-bulb temperature in degrees Celsius.

The value of  $A$  is dependent on, among other things, the ventilation rate past the wet-bulb thermometer. For Meteorological Office pattern mercury-in-glass thermometers in a standard thermometer screen with natural ventilation a value of 0.000799 is used. For psychrometers of the Assmann type with forced ventilation of 3 to 5  $\text{m s}^{-1}$  a value of 0.000667 is used. For practical purposes for readings from a fixed location at or near sea level, variations in atmospheric pressure may be ignored and  $P$  is taken as 1000 mb. Equation (8) thus reduces to

$$e = e_w' - 0.799(T - T_w) \quad \dots \quad (9)$$

for naturally ventilated psychrometers and

$$e = e_w' - 0.667(T - T_w) \quad \dots \quad (10)$$

for Assmann-type forced ventilation psychrometers.

### Relative humidity

Relative humidity is defined (World Meteorological Organization, 1975) by

$$U = 100 (e/e_a) \text{ per cent}, \quad \dots \quad (11)$$

where  $e$  = ambient vapour pressure in millibars, and

$e_a$  = saturation vapour pressure in millibars with respect to water at the same pressure and temperature.

### Computation of dew-point temperature

The dew-point temperature of a sample of air is defined as the temperature to which the sample must be cooled at constant pressure in order that the sample shall be saturated with respect to a plane surface of pure water. Thus the vapour pressure of the sample is the saturation vapour pressure at the dew-point temperature. If the ambient vapour pressure is known it is required to determine the temperature for which that is the saturation vapour pressure.

There is a variety of methods, based on the above equations for saturation vapour pressure, by which the dew-point temperature may be calculated. The Goff-Gratch equation is not amenable to easy solution for temperature, although for determining the accuracy of other methods it is possible by means of a reiterative trial and error method. This is very costly in terms of computing time.

*Richards.* Richards's equation (equation (6)) can be solved by an iterative process as follows:

Form a first estimate of  $t$  by neglecting all but the first term in equation (6); that is

$$t_1 = \frac{\ln(e/1013.25)}{13.3185} \quad (e = \text{vapour pressure in millibars}).$$

Successively more accurate estimates of  $t$  are obtained from

$$t_{n+1} = t_1 + [(0.1299t_n + 0.6445)t_n + 1.976] t_n^2 / 13.3185, \quad n = 1, 2, \dots$$

When the difference between successive estimates is suitably small the desired estimate of  $T_d$  (dew-point temperature) is found from

$$T_d = 373.15 / (1 - t_{n+1}).$$

For accuracy within the limitations of normal thermometry the difference between successive estimates should be less than  $10^{-8}$ .

*Lowe.* Lowe's polynomial (equation (7)) can be solved using the Newton-Raphson approximation method. This states that if  $q_n$  is an approximation to a root of a polynomial  $F(x)$  then a better approximation  $q_{n+1}$  is given by

$$q_{n+1} = q_n - F(x)/F'(x),$$

where  $F'(x)$  is the first derivative of  $F(x)$ .

The initial approximation,  $q_1$ , can be made by subtracting the wet-bulb depression from the wet-bulb temperature (e.g. dry bulb =  $10^\circ\text{C}$ , wet bulb =  $7^\circ\text{C}$ ; then first approximation to  $q = 7 - (10 - 7) = 4^\circ\text{C}$ ). Then

$$q_{n+1} = q_n - \frac{[(C_0 - e) + C_1q_n + C_2q_n^2 + \dots + C_6q_n^6]}{(C_1 + 2C_2q_n + 3C_3q_n^2 + \dots + 6C_6q_n^5)} \dots \dots \dots (12)$$

As successive approximations to  $q$  approach the limiting value the expression in the square brackets in equation (12) approaches zero and when it has become suitably small (less than  $10^{-8}$ )  $q_{n+1}$  may be taken as the dew-point temperature.

*Hooper.* Hooper has developed a sixth-order polynomial to compute temperatures from saturation vapour pressures. This is claimed to be valid from  $-40^\circ\text{C}$  to  $+50^\circ\text{C}$ .

$$T = n_0 + n_1V + n_2V^2 + n_3V^3 + n_4V^4 + n_5V^5 + n_6V^6, \quad \dots \dots \dots (13)$$

where  $T$  = temperature in degrees Celsius

$V$  = natural logarithm of the saturation vapour pressure in millibars

$$\begin{aligned}n_0 &= -2.25949867 \times 10^1 \\n_1 &= 1.133464632 \times 10^1 \\n_2 &= 5.754465709 \times 10^{-1} \\n_3 &= 2.999976429 \times 10^{-2} \\n_4 &= 1.971073756 \times 10^{-3} \\n_5 &= 3.369657915 \times 10^{-5} \\n_6 &= 1.384040269 \times 10^{-5}.\end{aligned}$$

The maximum departure from Goff-Gratch is  $1.40 \times 10^{-3} \text{ }^\circ\text{C}$  at  $+50 \text{ }^\circ\text{C}$  over the range  $-40 \text{ }^\circ\text{C}$  to  $+50 \text{ }^\circ\text{C}$ .

Hooper's coefficients can again be adjusted to give a closer approximation to the Goff-Gratch results:

$$\begin{aligned}n_0 &= -2.259529963 \times 10^1 \\n_1 &= 1.133418988 \times 10^1 \\n_2 &= 5.756940348 \times 10^{-1} \\n_3 &= 3.025080051 \times 10^{-2} \\n_4 &= 1.778276954 \times 10^{-3} \\n_5 &= 7.443287646 \times 10^{-5} \\n_6 &= 1.129170314 \times 10^{-5}.\end{aligned}$$

These coefficients give a maximum departure from Goff-Gratch of  $1.11 \times 10^{-4} \text{ }^\circ\text{C}$  at  $+50 \text{ }^\circ\text{C}$  over the range  $-40 \text{ }^\circ\text{C}$  to  $+50 \text{ }^\circ\text{C}$ .

*Polynomial in  $e$ .* It is possible to form a polynomial that approximates the curve of temperature against saturation vapour pressure with respect to water. A sixth-order polynomial is required to obtain an accuracy within the limits of normal thermometry over the range  $-10 \text{ }^\circ\text{C}$  to  $+35 \text{ }^\circ\text{C}$ .

$$T_d = A_0 + A_1 + A_2e^2 + A_3e^3 + A_4e^4 + A_5e^5 + A_6e^6, \quad \dots \quad (14)$$

where  $T_d$  = dew-point temperature in degrees Celsius

$e$  = ambient vapour pressure in millibars

$$\begin{aligned}A_0 &= -2.298609588 \times 10^1 \\A_1 &= 5.639954495 \\A_2 &= -4.058494504 \times 10^{-1} \\A_3 &= 1.839604600 \times 10^{-2} \\A_4 &= -4.669899615 \times 10^{-4} \\A_5 &= 6.085695266 \times 10^{-6} \\A_6 &= -3.160820732 \times 10^{-8}.\end{aligned}$$

The maximum departure from Goff-Gratch over the range is  $-3.757 \times 10^{-1} \text{ }^\circ\text{C}$  at  $+33.9 \text{ }^\circ\text{C}$ .

*Straight-line approximations.* The foregoing procedures are capable of giving accuracies within the limits of normal thermometry and, in the case of the logarithmic polynomials, considerably better. However, there may be applications where such a high degree of accuracy is not required and where ease of computation is a higher priority. For these applications it is possible to fit a series of straight lines to the curve of saturation vapour pressure against temperature. To achieve an accuracy of approximately  $\pm 0.3 \text{ }^\circ\text{C}$  in dew-point temperature over the temperature range  $-10 \text{ }^\circ\text{C}$  to  $+35 \text{ }^\circ\text{C}$  nine straight lines are required. These are of the form

$$e_w = B_0 + B_1T, \quad \dots \quad (15)$$

where  $e_w$  = saturation vapour pressure in millibars with respect to a plane surface of pure water at temperature  $T$ ,

$T$  = temperature in degrees Celsius

and

$T$ °C	$B_0$	$B_1$	$e_w$ mb
— 10 to — 5	+ 5.5388	0.2709	2.9 to 4.2
— 5 to 0	+ 6.0673	0.3792	4.2 to 6.1
0 to + 5	+ 6.0506	0.5231	6.1 to 8.7
+ 5 to + 10	+ 5.0874	0.7116	8.7 to 12.3
+ 10 to + 15	+ 2.6232	0.9554	12.3 to 17.0
+ 15 to + 20	— 2.0835	1.2671	17.0 to 23.4
+ 20 to + 25	— 9.9993	1.6610	23.4 to 31.7
+ 25 to + 30	— 22.3562	2.1535	31.7 to 42.4
+ 30 to + 35	— 40.6943	2.7630	42.4 to 56.2

Equation (15) may be rearranged thus:

$$T = \frac{e_w - B_0}{B_1}, \text{ or } T_d = \frac{e - B_0}{B_1}, \quad \dots \dots \dots (16)$$

to give dew-point temperature from ambient vapour pressure.

*Dew-point temperature from relative humidity.* The following expression will give dew-point temperature from values of relative humidity and dry-bulb temperature. It is reasonably accurate at high humidities with dry-bulb temperatures near to + 20 °C, but accuracy falls off quite rapidly as the relative humidity falls and the dry-bulb temperature departs from 20 °C.

$$T_d = T - K_0 + K_1 U, \quad \dots \dots \dots (17)$$

where  $T_d$  = dew-point temperature in degrees Celsius,

$T$  = dry-bulb temperature in degrees Celsius, and

$U$  = relative humidity, per cent

and

$U$	$K_0$	$K_1$
100 to 65 per cent	17.9	0.18
65 to 45 per cent	22.5	0.25

More accurate results are obtained from

$$T_d = U(0.198 + 0.0017T) + 0.84T - 19.2. \quad \dots \dots \dots (18)$$

This gives dew-point temperatures within 1 °C of the true value over ranges of dry-bulb temperature and relative humidity 0 °C to + 30 °C and 100 per cent to 40 per cent, respectively.

### Practical procedures

There are numerous ways of calculating the desired humidity variables of the air given the dry- and wet-bulb temperatures. The method used will depend on the accuracy required, the calculating facilities available and the preference of the user.



For all calculations it is necessary to compute the saturation vapour pressure at the wet-bulb temperature and this may be done using one of equations (1), (3), (5), (6) or (7) or the straight-line approximations—equation (15). The ambient vapour pressure may then be computed using equation (9) or (10).

If relative humidity is required, saturation vapour pressure at the dry-bulb temperature is computed, again from equations (1), (3), (5), (6), (7) or (15), and relative humidity is then found by means of equation (11).

If dew-point temperature is required the iterative solution of Richards's approximation or of Lowe's approximation, or the use of Hooper's polynomial (equation (13)) give the most accurate results. Alternatively the polynomial in  $e$  (equation (14)) or the straight-line approximation (equation (16)) may be used, giving a lower degree of accuracy.

If dry-bulb temperature and relative humidity are given, equations (17) and (18) will give a value of dew-point temperature if great accuracy is not important.

### Comparison of methods

In order to compare one method against another the Goff–Gratch results were taken as standard and other methods compared with them.

Table I gives values of mean relative difference, root-mean-square relative difference and maximum difference from Goff–Gratch for Hooper's, Hooper's with Sargent's coefficients, Richards's, Lowe's and straight-line approximations for saturation vapour pressure from temperature.

Table II gives similar values for Richards's, the Newton–Raphson solution of Lowe's polynomial, Hooper's, Hooper's with Sargent's coefficients, the polynomial in  $e$  and straight-line methods of obtaining temperature from values of saturation vapour pressure.

The values in Tables I and II were computed over the stated range of temperature at 0.1 °C increments.

**Table I.** *Comparison of computation methods for saturation vapour pressure from temperature against Goff–Gratch*

Method	Range of temperature degrees Celsius	Mean relative difference	Root-mean-square relative difference millibars	Maximum relative difference
Hooper	—40 to + 50	$1.564 \times 10^{-5}$	$4.087 \times 10^{-5}$	$2.106 \times 10^{-4}$ at —40.0 °C
Sargent	—40 to + 50	$-3.889 \times 10^{-6}$	$5.016 \times 10^{-6}$	$-1.522 \times 10^{-6}$ at +50.0 °C
Richards	—40 to + 50	$1.495 \times 10^{-4}$	$3.369 \times 10^{-4}$	$7.819 \times 10^{-4}$ at —34.3 °C
Lowe	—40 to + 50	$3.079 \times 10^{-4}$	$7.711 \times 10^{-4}$	$2.408 \times 10^{-3}$ at —33.5 °C
Straight-line approximations	—10 to + 35	$-1.485 \times 10^{-3}$	$4.134 \times 10^{-3}$	$1.149 \times 10^{-2}$ at —35.0 °C

**Table II.** *Comparison of computation methods for temperature from saturation vapour pressure against Goff–Gratch*

Method	Range of temperature	Mean relative difference	Root-mean-square relative difference	Maximum relative difference
	degrees Celsius			
Richards	—40 to + 50	$4.141 \times 10^{-3}$	$5.629 \times 10^{-3}$	$-3.971 \times 10^{-3}$ at + 50 °C
Lowe	—40 to + 50	$3.218 \times 10^{-3}$	$8.013 \times 10^{-3}$	$3.739 \times 10^{-3}$ at —12 °C
Hooper	—40 to + 50	$-1.349 \times 10^{-4}$	$4.518 \times 10^{-4}$	$1.401 \times 10^{-3}$ at + 50 °C
Sargent	—40 to + 50	$5.750 \times 10^{-5}$	$6.396 \times 10^{-5}$	$1.113 \times 10^{-4}$ at + 50 °C
Polynomial in $e$	—10 to + 35	$5.587 \times 10^{-3}$	$1.317 \times 10^{-1}$	$-3.757 \times 10^{-1}$ at + 33.9 °C
Straight-line approximations	—10 to + 35	$2.285 \times 10^{-2}$	$5.938 \times 10^{-2}$	$-1.214 \times 10^{-1}$ at —10 °C

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- |                                   |      |  |
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551.5(09):551.508:069

## The Museum of Meteorological Instruments

By J. P. Jay

(Meteorological Office, Bracknell)

‘The thing that hath been, it is that which shall be; and that which is done is that which shall be done: and there is no new thing under the sun.’

*Ecclesiastes 1:9.*

Anyone seeking to prove the wisdom of this well-known quotation might be advised to visit the Museum of Meteorological Instruments (Plate II). There he will find many examples of old instruments, the principles and often the looks of which are disconcertingly similar to those of current models. This, the Meteorological Office's own Museum, was opened in December 1979, in Room 402 (off the Staff Restaurant) in the Headquarters Building at Bracknell. It is open for viewing during normal working hours when Meteorological Office staff are at liberty to walk round at leisure. Members of the general public may also be shown round on application at the main entrance.

It is very important that steps should be taken to preserve examples of instruments before they disappear for ever. Apart from their obvious interest value, it is often very useful for designers to be able to refer to what has gone before; a look at the real thing can be far more helpful than drawings and many pages of descriptive writing. The present collection is just a beginning. The exhibits will increase in number as the years go by. Now that the initiative has been taken, it is to be hoped that all those who have meteorological instruments in their care will ensure that any of an old or interesting nature will be offered to the Museum before being disposed of elsewhere. If contact is made with Met 0 16c, arrangements will be made to collect anything suitable.

The instruments on display include many which are worthy of special mention. Impressive at the far end of the room, stands the massive Beckley-Robinson anemograph which was installed on the roof

of the Radcliffe Observatory, Oxford, in 1893 (Plate III). It worked so reliably and effectively that it was in constant use for 83 years until 1976 and the clock mechanism still functions perfectly. It is on permanent loan from the University of Oxford School of Geography. This piece of equipment was designed by Robert Beckley, of Kew Observatory, in 1856. He used the cup anemometer originated by Dr Thomas Romney Robinson, the Irish astronomer, some ten years earlier. The characteristic direction mechanism was based on the windmill governor invented by Follet Osler. This gives a much steadier indication of wind direction than a plain wind vane and Beckley improved Osler's design by using two windmills instead of one.

In the case alongside will be seen an original early prototype model which was constructed by Beckley himself (Plate IV). The modern Mk 4E wind vane adjacent to it looks remarkably similar, showing how difficult it is, even in this modern age, to improve on the work of the 'old masters' (Plate V).

It is not generally realized that the tipping-bucket rain-gauge dates back as far as 1660 when no less a person than Sir Christopher Wren used the system. The principle has survived the years and remains the basis of quite sophisticated modern instruments. Various examples are on show. Of particular interest is the Negretti & Zambra improved self-recording rain-gauge (c. 1890) which was kindly presented to us by the East Devon District Council (Plate VI). This unit was in regular use at the Sidmouth climatological station until 1973. In this device each tip of the buckets, holding 0.01 inch, advances an escapement wheel and a trace is made on the drum of a clockwork recorder. Another recording rain-gauge of about the same age is the Negretti & Zambra dial rain-gauge where tips are mechanically transmitted to a pointer and dial in 0.01 inch increments (Plate VII).

In the field of humidity measurement we have examples of dew-point hygrometers such as Daniell's (1860) (Plate VIII), G. Dines's and Regnault's (1875), all of which use methods of cooling a glass surface so that the temperature at which condensation occurs may be observed. Today's dew-point hygrometers do exactly the same but with automatic optical aids to perform the observation. Horace Benedict de Saussure, of Geneva, first suggested the use of human hair for determining humidity. An interesting example of an early hair hygrometer by Bate (c. 1830) is on display. It hardly needs to be pointed out that Meteorological Office hygrographs in 1980 operate on the same principle. In the novelty class and not, to my knowledge, copied today, is Kater's hygrometer (c. 1812) (Plate IX); this takes advantage of the twisting action of the seed awn of the grass *Andropogon contortum* to operate a pointer over a graduated dial.

Wet- and dry-bulb thermometers are still the 'standard' for humidity observations and have been in use for some considerable time. The fact that a wet thermometer bulb gives a lower reading than a dry one was first noted by William Cullen, a Scot, in 1777. It was not until 1792 that James Hutton, a compatriot, used the principle for hygrometry. Dr Abraham Mason produced his well-known psychrometer in 1836 and the Museum has a neat example dated around 1870. An instrument for remote wet- and dry-bulb observations is the Negretti & Zambra recording thermometer (c. 1898) (Plate X). This incorporates an electrical system, actuated by a clock, to 'flip over' a pair of constricted thermometers and thus hold their readings so that observations for a specific time can be taken at leisure.

Another early instrument, obviously intended for remote use, is the Goldschmid barograph (1880) (Plate XI). This has an aneroid capsule stack and a clock mechanism. At hourly intervals indentations are punched on to a paper roll from which pressures can be read off later, using a calibrated scale. Aneroid capsules still dominate the techniques of practical barometry, having almost entirely pushed out the once-supreme liquid types. On the left as one enters the Museum is the top part of the 1873 Jordan glycerine barometer (Plate XII) which was installed in the office of *The Times* at Printing House

Square, London, until about 1957. As the tube was some 30 feet long, the cistern would have been housed two or three floors below. The readings were plotted on weekly charts in the very substantial shuttered cabinet alongside. The record displayed is presumably the last one made before the equipment was abandoned. The resolution of this barometer was apparently very fine and one wonders whether adequate practical use could ever have been made of the information. The oldest and most treasured item in our collection is the mercury barometer by Dan Quare (c. 1695) (Plate XIII). This superb instrument, which is still in good working order, is truly a fitting memorial to the craftsmanship of the period. It is currently kept in the office of the Assistant Director (Operational Instrumentation) at Beaufort Park, where it may be seen by arrangement with his personal assistant. Other interesting articles at present housed outside the Museum itself include a Dines's improved hygrometer and a set of genuine African rain-making stones which are in the Director-General's office.

Much has been written about the FitzRoy storm glass but no one has yet ventured any explanation of how it works. I do not intend to break with that tradition, but we have reconstructed one, using a formula handed down through the years. Although it is housed in the relatively stable environment of the display cabinet, regular visitors will see that interesting changes in the crystalline formations do take place. It has, so far, been impossible to relate these clearly to weather changes for prognostic purposes as Admiral FitzRoy so confidently did. Its presence in the Museum takes us back hundreds of years to the days of the old alchemists and the times when interest in instruments to assist attempts to monitor and forecast the weather was just beginning. Perhaps our instrument designers of today may take heart from the progress that has been made and whilst not forgetting the well-tried principles, nevertheless realize that there is ample room for many really new developments for the sun to shine on.

## Review

*A short course in cloud physics (second edition)*, by R. R. Rogers. 215 mm × 150 mm, pp. viii + 235, illus. Pergamon Press, Oxford, New York, Toronto, Sydney, Paris, Frankfurt, 1979. Price (flexicover) US \$12.50, £6.25 (hardback available at US \$25.00, £12.50).

The first edition of this monograph was published some four years ago and assessed by the present reviewer (*Meteorol Mag*, 105, 1976, pp. 216–217). Some minor errors have been corrected in the second edition. Thus the values given for a number of constants and parameters of interest are now in keeping with those of standard tables. Greater consistency in the use of SI units is apparent; nevertheless some lapses remain. A small amount of new material has been added in the sections on condensation and ice nucleation. Derivations of the condensational and coalescence growth equations have been recast. However, the major improvement is the provision of hints to solutions and answers for alternate problems set at the end of each chapter. This corrects a rather obvious deficiency and almost certainly increases the value of the book as a teaching aid.

It must be admitted that a criticism levelled at the first edition has not been fully justified in practice. It was suggested that the book was unlikely to provide a text for students which was comprehensive enough to be a useful addition to the standard works. Whilst some further expansion of the sections dealing with theoretical developments would be an advantage, the reviewer's copy of the first edition has been consulted frequently as an *aide-mémoire*. Rogers has succeeded in distilling much that is of practical value in the subject.

P. Ryder



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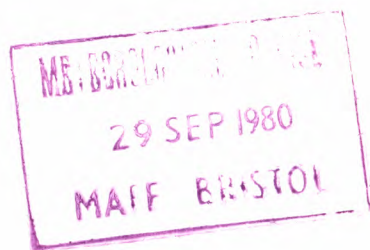
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 8/80

Annual subscription £21.18 including postage  
ISBN 0 11 722064 7  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

September 1980

Met.O. 931 No. 1298 Vol. 109





# THE METEOROLOGICAL MAGAZINE

No. 1298, September 1980, Vol. 109

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551.506.3(424)

## **A homogeneous rainfall record for the Cirencester area, 1844–1977**

By P. D. Jones

(School of Environmental Sciences, University of East Anglia, Norwich)

### **Summary**

Monthly rainfall records for the Cirencester area have been analysed for the years 1844–1977. A series of monthly totals comparable with those for the observing station at Baunton Waterworks has been produced. Comparisons with the rainfall record from Oxford are made to verify the homogenization.

### **Introduction**

Cirencester provides a wealth of material for the production of a composite rainfall record representing the town. All years since 1859, except 1884, have at least two sites operating within the town, and the neighbouring towns of Cheltenham and Oxford provide reliable values for comparison purposes. The town is situated in the extreme headwaters of the River Thames just to the south of the main Cotswold ridge, approximately midway between Swindon and Cheltenham.

### **Availability of rain-gauge records**

The map shown in Figure 1 marks the location of each rain-gauge referred to in Table I and the temporal extents of the various records are listed in this table. As can be seen from the map, most of the gauges are to the north and west of the town, particularly off the main road to Cheltenham which runs north-westwards from the town.

### **Choice of key site and methods of comparison**

The gauge at Baunton Waterworks is ideal for a key site as it is likely to be free of obstruction and is likely to remain in existence for some time to come. It is interesting to note that the earliest gauge in operation near Cirencester, Further Barton, was situated within a mile of the key site at Baunton.

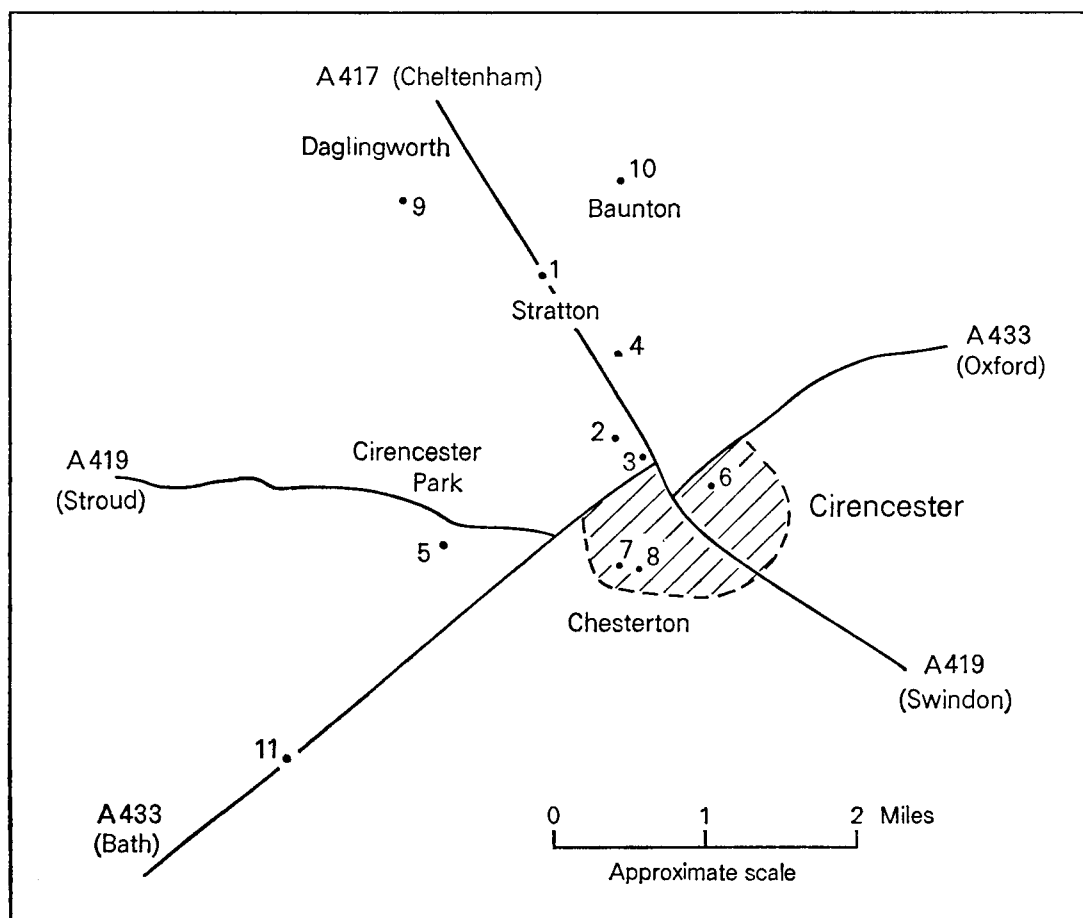


Figure 1. Rain-gauges in the Cirencester area (numbers refer to gauges in Table I).

Table I. *Gauges within the Cirencester area (1844–1977)*

Gauge name	Location number (Figure 1)	First year	Last year	Missing years	Altitude (m)	Observer
Further Barton	1	1844	1909	1884	129	J. G. Brown 1844–83*
Cripps Mead	2	1902	1929	—	111	Wilfred Cripps
Dollarwood House	3	1889	1924	—	111	C. P. Hooker
'The Firs'	4	1869	1883	—	107	J. Bravender
Royal Agricultural College	5	1875	1914	—	135	Principal
Gwynfa	6	1923	1941	—	108	N. Woods
Somerford Road	7	1942	1956	—	115	
Chesterton Grove	8	1957	1977	—	123	
Daglingworth Manor	9	1924	1951	—	137	J. Herbert Scotton
Baunton Waterworks	10	1948	1977	—	121	D. W. Harvey
Thameshead	11	1859	1869	—	?	Cirencester UDC J. H. Tannton

\* also Miss J. Brown (1885–1905) and C. P. Hooker (1906–09).

The method employed in this study is that of 'annual ratios' described by Craddock (1977).<sup>\*</sup> The ratio of the year's catches at two rain-gauges is plotted against time and, in the ideal situation, the plotted points should vary about a straight line. The degree of variation and the level of the line relative to unity will naturally depend on the distance between the rain-gauge sites and the type of terrain within which the gauges are situated. Ratios between gauges within Cirencester should be near unity, whereas those between gauges some distance apart should vary about the ratio between the gauges' average annual catches.

Any climatic change affecting Cirencester town can be assumed to affect the region around. If, then, the ratios tend to deviate, the catch of either gauge, or the catches of both gauges, must have been affected by

- (a) a change in site of either gauge, or
- (b) a change in either gauge's local environment, generally a deterioration due possibly to vegetal growth, building construction, etc.

Changes due to (a) will generally be abrupt, while those due to (b) will be more gradual and take place over a length of time. With many gauges operating within Cirencester since the 1870s, it should be possible to discover which gauge is at fault, should any deviation from a straight line occur.

### **Construction of a composite record**

Unlike many sites for which composite records have been produced (e.g. Norwich (Craddock, 1977)), no one site has a continuous record for more than about 30 years except Further Barton. However, many records exist and it is necessary to choose the best of those available. The major early gauge is at Further Barton. This was the only one in existence until 1859, and it is therefore the only one available for the first 15 years. Comparison of the Further Barton gauge with others in the town, at Oxford (Figure 2(a)) and at Cheltenham between 1844 and 1909, reveals three distinct periods: 1844–58, 1859–79 and 1880–1909. For the first and last periods the ratios remain steady, but at different reference levels, and are extremely consistent with other gauges, particularly those at Dollarwood House, Cripps Mead and the Royal Agricultural College (Figure 2(b)). However, the period between 1859 and 1879 is marked, firstly by an increase in rainfall caught, and then by a decline from 1870 to 1879. This latter decline is particularly evident when the Further Barton record is compared with that of 'The Firs' (Figure 2(c)), which is less than half a mile away, and with Oxford (Figure 2(a)). The decrease in catch at Further Barton from 1870 to 1879 can be explained by the infirmity of the observer. In the 10-year sheets stored at the Meteorological Office, the extreme age of the observer is commented upon on at least two occasions during the 1870s. He was possibly unable to cut down vegetation growth causing the gauge to be over-sheltered. It is therefore necessary to use 'The Firs' monthly totals for the period until 1882. For the period 1859–68 the Thameshead record is preferred to that at Further Barton. Comparison (Figure 3(a)) of the Thameshead gauge with that at Further Barton points to discrepancies in the latter gauge during this period as noted in comparisons with Oxford (Figure 2(a)).

After 1883, when the excellent record from the gauge at 'The Firs' ceases, 1884 has to be filled by the Royal Agricultural College, as it was the only gauge operating. From 1885 until the late 1920s there are generally three gauges in operation. The Royal Agricultural College gauge, when compared with Further Barton (Figure 2(b)), Cripps Mead (Figure 3(b)) and Dollarwood House, reveals very good internal consistency between 1883 and 1909. The gauge is also the only one that overlaps with 'The

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<sup>\*</sup> Craddock, J. M. A homogeneous record of monthly rainfall totals for Norwich for the years 1836 to 1976. *Meteorol Mag*, 1977, 106, 267–278.

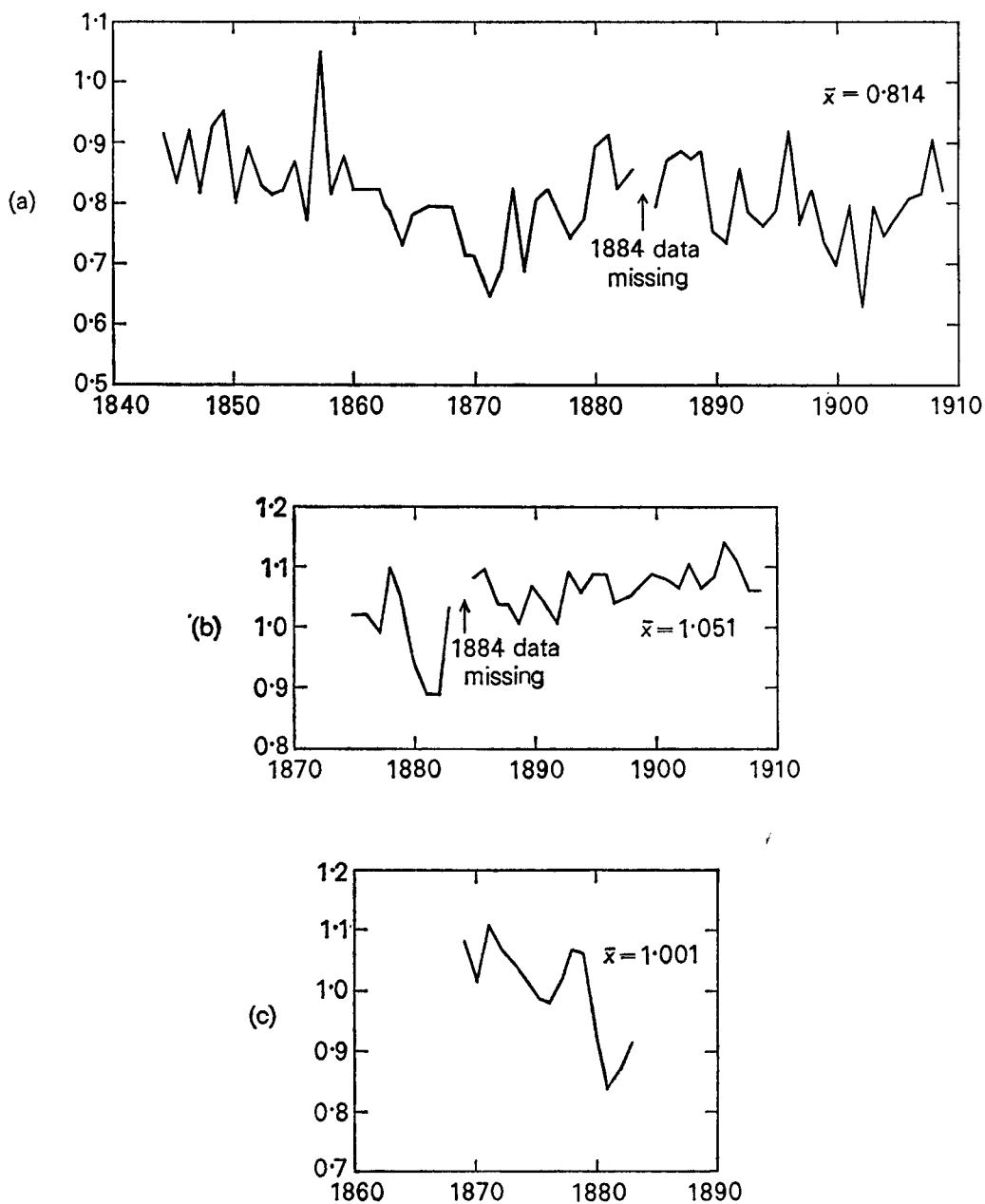


Figure 2. Ratios of annual catches: (a) Oxford/Further Barton, 1884–1909; (b) Further Barton/Royal Agricultural College, 1875–1909; (c) Further Barton/'The Firs', 1869–83.

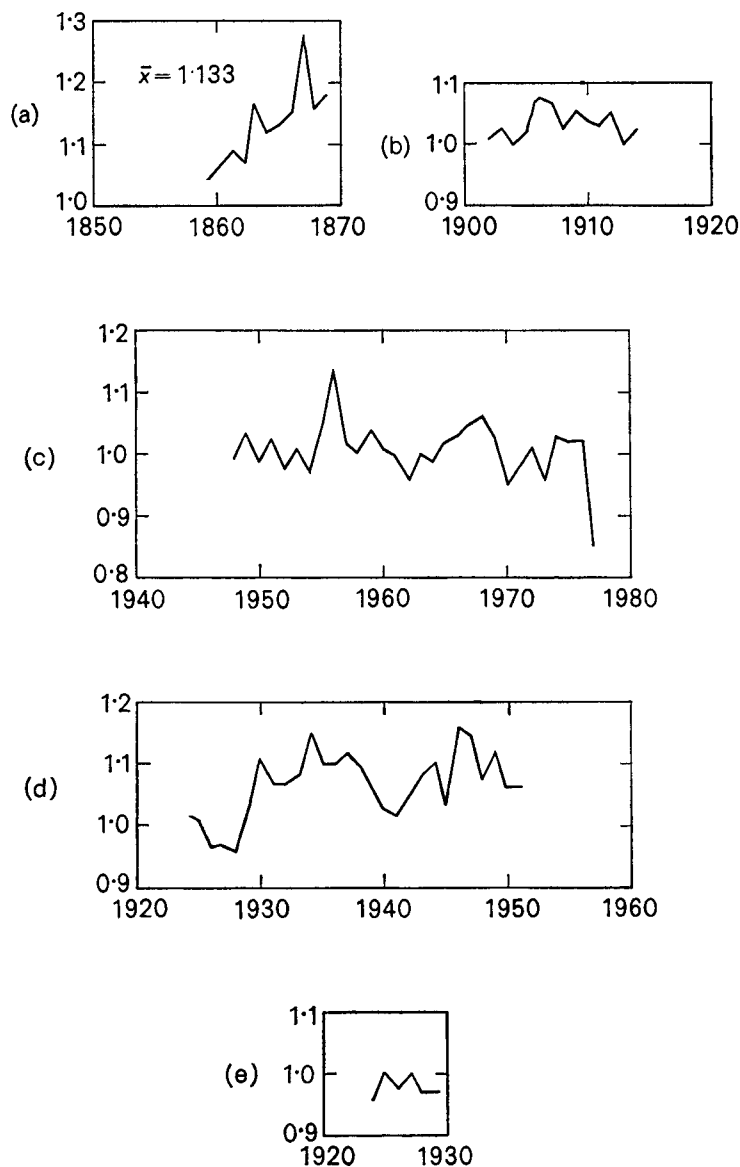


Figure 3. Ratios of annual catches: (a) Further Barton/Thameshead, 1859-69; (b) Cripps Mead/Royal Agricultural College, 1902-14 ( $\bar{x} = 1.034$ ,  $\sigma = 0.025$ ;  $\bar{x} = 1.022$ ,  $\sigma = 0.017$  (omitting 1906-09)); (c) Baunton/Cirencester (Woods), 1948-77 ( $\bar{x} = 1.008$ ,  $\sigma = 0.048$ ;  $\bar{x} = 1.009$ ,  $\sigma = 0.029$  (omitting 1956 and 1977)); (d) Daglingworth Manor/Cirencester (Woods), 1924-51 ( $\bar{x} = 1.089$ ,  $\sigma = 0.037$  (1930-51 omitting 1941)); (e) Cripps Mead/Cirencester (Woods), 1924-29 ( $\bar{x} = 0.981$ ,  $\sigma = 0.017$ ).

Firs' before 1882 and with Cripps Mead from 1902 to 1915 (since the Further Barton gauge records for the period 1870–80 have been shown to be in error). This record was, therefore, used until 1910, when the Cripps Mead record until 1929 took its place. The Cripps Mead record is excellent except for the years 1906–09, when the ratio with the Royal Agricultural College (Figure 3(b)) is 5 per cent above the value for 1910–14. This is also found in comparisons between Dollarwood House and Cripps Mead, which explains why the break between the Royal Agricultural College and Cripps Mead was chosen at 1909.

Mr N. Woods has maintained gauges at three locations since 1923, but all his sites are to the south of the town, and it would be preferable to use sites to the north-west, near to the key site of Baunton and the early records at Further Barton. Annual ratio graphs are given of the amalgamation of Mr Woods's three sites with Baunton Waterworks, Daglingworth Manor and Cripps Mead (Figures 3(c)–(e)) during the respective overlap periods. Discrepancies that are revealed in 1941 and 1956 can be explained by Mr Woods's relocation of instruments. The records from Daglingworth Manor up until 1929 also appear somewhat in error and explain the necessity for using Cripps Mead up until 1929. The final composite record is listed below. Details of the production of the appropriate factors can be found in Appendix 1.

Period	Station	Factor
1844–58	Further Barton	1.070
1859–68	Thameshead	1.108
1869–82	'The Firs'	0.954
1883–1909	Royal Agricultural College	1.052
1910–29	Cripps Mead	1.029
1930–47	Daglingworth Manor	0.926
1948–	Baunton Waterworks	1

### Concluding remarks

Monthly values of the composite record for Cirencester are given in Appendix 2 and the ratio of the Oxford record to the new one at Cirencester is plotted for the period 1844–1975 (Figure 4). As the

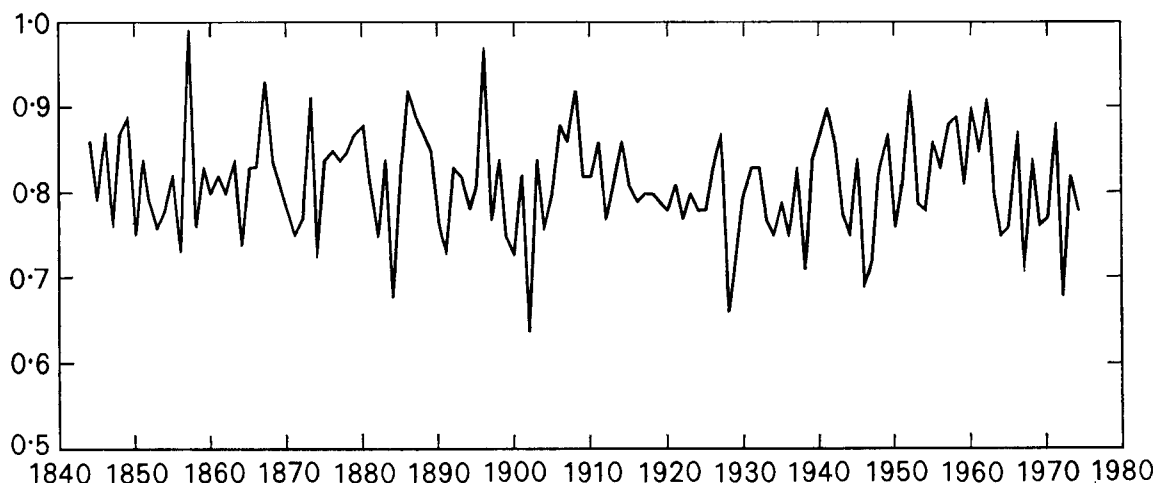


Figure 4. Ratios of annual catches Oxford/Cirencester.

Further Barton gauge is used for the first five years, the curve is the same as in Figure 2(a) albeit slightly shifted (owing to the multiplying factor of 1.070). For the period 1844–1975 the average annual ratio between Oxford and Cirencester is 0.811. This compares very favourably with the Oxford/Baunton ratio for 1948–74 of 0.818 and mean of the 1941–70 averages for Oxford and Baunton of 0.808.

It was not possible to incorporate earlier records extant for the region owing to the lack of overlap with gauges in the Cirencester area. The best record of this type is that kept at Stroud by Dr T. Hughes from 1771 to 1812. Short records (less than five years) are available in the towns of Bristol, Cheltenham and Gloucester but the gaps which would have to be filled from Oxford are considered to be too great.

### Acknowledgements

The author wishes to thank the staff of the Agriculture and Hydrometeorology Branch (Met O 8) and the Archives (Met O 18e) of the Meteorological Office, Mr J. M. Craddock and Dr T. M. L. Wigley for useful discussions relating to the production of the composite record. The author also acknowledges the support of the Department of the Environment Research Contract No. 116/2/2.

### Appendix 1. The production of the composite record

#### 1844–1958 *Further Barton*

The average ratio for Oxford/Baunton for 1948–74 is 0.818, with a standard deviation ( $\sigma$ ) of 0.062. (The standard deviations between Cirencester gauges and Oxford are much higher than those within Cirencester owing to the greater distance between the gauges.)

Oxford/Further Barton (1844–58) = 0.875,  $\sigma$  = 0.074.

Thus conversion to Baunton =  $0.875/0.818 = 1.070$ .

#### 1859–68 *Thameshead*

Oxford/Thameshead (1858–69) = 0.906,  $\sigma$  = 0.054.

Thus conversion to Baunton =  $0.906/0.818 = 1.108$ .

#### 1869–82 *'The Firs'*

As the early years at the Royal Agricultural College (Figure 2(b)) are suspect, this ratio was produced from Oxford.

Oxford/'The Firs' (1869–83) = 0.780,  $\sigma$  = 0.050.

Thus conversion to Baunton =  $0.78/0.818 = 0.954$ .

#### 1883–1909 *Royal Agricultural College*

Cripps Mead/Royal Agricultural College (1902–14 omitting 1906–09) = 1.022,  $\sigma$  = 0.017.

Cripps Mead (1910–29) correction factor = 1.029.

Thus conversion to Baunton =  $1.029/1.022 = 1.052$ .

#### 1910–29 *Cripps Mead*

Baunton/Mr Woods's gauges (1948–74, omitting 1956) = 1.009,  $\sigma$  = 0.029.

Cripps Mead/Mr Woods's gauge (1924–29) = 0.981,  $\sigma$  = 0.017.

Thus conversion to Baunton =  $1.009/0.981 = 1.029$ .

#### 1930–47 *Daglingworth Manor*

Daglingworth Manor/Mr Woods's gauges (1930–51) = 1.089,  $\sigma$  = 0.037.

Thus conversion to Baunton =  $1.009/1.089 = 0.926$ .

It is worth mentioning the size of the standard errors of the estimates of these ratios. For a normal distribution the standard error is  $\sigma/n^{1/2}$ . Thus approximations for the standard errors of the conversion factors can be made. For those involving comparisons with Oxford, Further Barton, 'The Firs' and Thameshead they are in the range 0.03 to 0.04 and thus the third decimal place is not warranted. However, for those computed within Cirencester they are of the order of 0.01, thus justifying the third decimal place.

Appendix 2. A monthly rainfall record for the Cirencester region (units are tens of millimetres).

	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Annual
1844	434	1033	462	55	27	190	544	462	489	884	1237	150	5967
1845	517	272	353	517	679	978	761	807	856	408	815	897	7860
1846	1264	381	508	992	600	299	584	1209	394	1699	462	272	8664
1847	753	653	489	429	625	789	388	250	706	1585	690	1046	8403
1848	332	1101	870	951	122	1332	837	789	1202	1373	446	987	10342
1849	522	476	245	842	911	468	326	253	1033	663	462	842	7043
1850	547	552	247	1177	951	296	1443	489	734	399	861	775	8471
1851	1101	231	1250	307	394	715	745	789	136	725	174	448	7015
1852	1549	462	136	190	544	1853	856	1541	1489	1055	2432	1168	13275
1853	1114	286	272	696	924	1033	1076	1011	508	1149	598	272	8939
1854	810	261	171	68	789	470	859	245	190	739	413	402	5417
1855	117	340	718	163	565	701	1318	557	617	1522	122	353	7093
1856	911	570	429	905	1073	346	333	1142	1073	911	299	794	8806
1857	824	422	530	706	478	794	671	353	489	1020	340	228	6855
1858	163	272	245	1427	720	865	544	535	799	478	226	696	6970
1859	422	433	724	735	397	948	394	720	859	766	673	771	7842
1860	735	568	692	428	1038	1725	453	1122	866	532	780	859	9798
1861	394	771	661	214	327	625	1061	146	768	413	1199	493	7072
1862	760	130	1351	695	1064	760	535	495	954	1115	194	464	8517
1863	828	155	304	318	298	996	124	830	833	1005	616	391	6698
1864	501	450	844	298	298	400	265	338	791	572	608	819	6184
1865	678	611	312	307	523	273	1379	1106	696	1475	886	439	8685
1866	1056	960	312	619	203	903	751	1013	1635	667	448	762	9329
1867	766	577	603	718	793	375	1044	658	402	560	276	594	7366
1868	968	391	676	543	391	113	121	1115	830	704	486	1568	7906
1869	1209	751	383	313	941	320	194	385	1405	477	603	1197	8178
1870	565	529	425	172	390	160	485	603	288	1027	458	552	5654
1871	439	368	354	812	378	719	1020	589	1357	371	165	530	7102
1872	1221	695	615	574	507	836	1127	793	390	884	1168	979	9789
1873	902	395	741	198	630	535	713	633	395	571	504	235	6452
1874	795	579	261	385	245	446	264	771	1320	923	681	674	7344
1875	1284	620	259	480	579	829	1347	293	678	1895	1221	398	9883
1876	596	1004	1064	899	157	354	238	727	1372	354	982	1827	9574
1877	1078	487	603	804	618	255	964	1529	508	565	1114	475	9000
1878	429	470	398	732	1125	730	223	1180	419	1042	826	429	8003
1879	884	1251	301	754	659	1180	914	1752	884	214	93	214	9100
1880	165	943	507	535	198	461	1773	387	1040	1399	739	903	9050
1881	363	1219	562	211	332	623	659	1301	543	494	1083	839	8229
1882	683	477	482	1056	507	921	1427	785	788	1524	1161	919	10730
1883	965	1013	291	168	385	1072	927	283	1104	716	1008	182	8114
1884	1010	406	612	380	264	793	970	569	387	278	497	986	7152
1885	711	930	307	564	620	451	142	572	1192	874	1473	264	8100
1886	1125	217	652	460	1199	248	863	462	700	1117	719	1128	8890
1887	644	165	401	340	432	369	254	427	719	665	679	441	5536
1888	241	500	956	358	588	708	1199	583	316	214	1510	866	8039
1889	214	420	821	732	986	270	751	949	460	602	422	457	7084



Appendix 2 continued

	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Annual
1890	845	144	246	363	618	638	946	510	345	387	623	236	5921
1891	777	11	476	302	1037	380	1039	1577	420	2138	644	944	9745
1892	276	473	171	251	248	684	863	815	807	796	682	281	6347
1893	588	741	67	19	398	281	762	612	160	770	500	639	5537
1894	633	628	697	706	484	583	722	676	885	1355	1317	732	9418
1895	770	24	628	503	248	201	1088	713	217	741	1392	583	7108
1896	171	102	649	219	75	555	174	607	1692	970	201	850	6265
1897	602	962	890	593	305	802	446	1553	671	444	409	1088	8765
1898	125	328	125	470	817	299	201	777	241	973	716	831	5903
1899	1008	812	128	473	746	449	270	444	697	756	724	652	7159
1900	807	1553	254	217	481	636	331	1026	107	772	697	1411	8292
1901	272	291	618	833	331	593	758	639	724	396	243	1275	6973
1902	297	302	538	548	505	798	283	944	481	620	706	679	6701
1903	863	380	1008	673	1197	1263	885	959	781	1806	668	510	10993
1904	930	1256	358	352	764	321	984	692	802	182	473	569	7683
1905	224	187	1095	708	78	1144	165	1165	331	387	986	224	6694
1906	430	1906	449	195	515	855	350	288	174	1390	770	503	7004
1907	227	358	229	798	602	628	1066	462	163	1483	583	1395	7994
1908	689	236	735	676	513	168	521	1039	519	473	375	612	6556
1909	335	115	991	494	417	1144	599	932	802	1251	238	1275	8593
1910	645	1095	154	792	382	1022	599	1008	105	837	1059	1338	9036
1911	290	486	509	270	282	494	13	321	358	842	938	1461	6264
1912	1529	543	1320	42	696	1040	949	1730	222	988	463	1283	10805
1913	1273	411	972	1156	548	306	258	361	687	821	805	387	7985
1914	227	946	1008	358	407	677	967	395	458	562	1168	1584	8757
1915	1152	1205	334	316	918	458	1265	523	379	1116	554	1738	9958
1916	648	1338	1020	321	708	774	492	964	345	1785	677	1152	10224
1917	478	353	735	345	742	769	653	1702	643	1059	219	270	7968
1918	1008	484	377	679	481	304	941	623	1728	515	693	884	8717
1919	1226	821	1184	703	379	400	653	643	478	402	426	1242	8557
1920	998	248	735	1220	512	818	1234	300	450	630	324	860	8329
1921	753	94	484	261	465	73	120	515	478	413	659	407	4722
1922	815	1012	706	726	295	295	1310	1268	623	285	445	1025	8805
1923	643	1505	611	684	405	100	484	577	713	1291	538	901	8452
1924	1056	162	397	735	1775	726	1163	1030	1129	1158	567	1207	11105
1925	630	1054	107	460	1015	29	808	1108	1037	986	538	991	8763
1926	1315	565	173	878	967	489	548	536	295	724	1843	110	8443
1927	857	1020	862	416	298	969	944	1370	1422	538	653	915	10264
1928	1278	713	803	345	277	867	684	554	214	1312	981	839	8867
1929	300	193	58	270	747	353	463	531	71	1168	1937	1845	7936
1930	1096	145	524	762	287	588	1045	771	1011	555	962	901	8647
1931	532	585	47	672	938	950	962	1289	588	186	1188	435	8372
1932	889	33	501	765	1545	332	576	498	837	1321	623	270	8190
1933	631	941	933	301	478	367	219	219	729	884	266	156	6413
1934	748	82	623	534	261	480	767	527	759	544	412	1792	7529
1935	202	858	157	1341	318	927	249	417	1515	1320	1644	955	9903
1936	1063	593	536	562	330	530	1512	153	1077	386	868	1061	8671
1937	1181	1312	1086	811	597	270	703	171	449	837	468	585	8470
1938	997	254	66	26	644	498	640	1115	892	1037	944	953	8066
1939	1581	400	579	929	247	275	1446	534	287	894	1397	657	9226

## Appendix 2 continued

	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Annual
1940	663	748	670	569	510	188	826	44	179	974	1757	371	7499
1941	948	795	672	278	383	431	767	1037	193	588	595	487	7174
1942	880	282	579	348	1141	28	621	1058	496	870	440	1005	7748
1943	1445	296	258	179	820	548	334	670	498	634	496	395	6593
1944	536	223	78	440	223	781	644	936	748	985	1385	498	7477
1945	532	696	301	315	576	990	344	571	468	663	92	1195	6743
1946	793	642	296	524	849	877	518	2046	1077	134	1573	632	9961
1947	581	339	1996	698	362	494	765	122	444	99	513	597	7010
1948	1318	353	368	612	1107	696	241	846	638	1054	399	1102	8734
1949	345	396	450	437	686	218	541	376	353	1694	790	345	6631
1950	145	1740	404	655	632	541	1034	1034	1222	224	1509	498	9638
1951	1059	975	1001	833	711	358	544	1417	965	264	1750	691	10568
1952	544	201	732	610	678	447	61	937	284	1064	886	734	7178
1953	206	414	310	671	620	472	871	1217	734	599	409	229	6752
1954	419	866	864	97	432	1214	653	859	958	864	1565	638	9429
1955	671	424	427	257	1161	1008	97	152	185	455	864	1067	6768
1956	965	58	152	602	137	607	1077	1262	1019	417	251	1069	7616
1957	876	1008	775	81	399	386	838	907	1176	528	455	683	8112
1958	813	1059	399	221	655	859	780	605	1280	650	815	884	9020
1959	1138	33	706	815	376	437	612	592	43	538	747	1593	7630
1960	1247	747	396	277	455	869	1191	1166	1024	1250	1273	813	10708
1961	996	673	13	1303	333	356	681	572	615	739	318	1105	7704
1962	1085	147	310	665	488	69	480	1184	869	249	691	696	6933
1963	254	175	1097	655	399	1143	554	886	422	544	1570	318	8017
1964	163	340	884	584	625	602	251	163	208	361	439	932	5552
1965	859	56	663	541	460	836	950	389	1097	196	765	1875	8687
1966	551	1069	277	965	625	328	688	1105	348	1102	582	1006	8646
1967	594	1120	615	320	1702	292	470	447	1171	1725	574	749	9779
1968	909	470	302	838	714	1011	937	587	1455	798	734	963	9718
1969	777	551	625	381	1171	399	577	1313	224	76	965	927	7986
1970	947	627	490	668	305	658	691	617	818	348	1727	404	8300
1971	1392	366	620	546	627	1247	351	1039	203	917	688	409	8405
1972	940	970	635	551	808	521	297	384	396	348	737	1829	8416
1973	399	282	185	660	706	671	828	485	648	269	442	452	6027
1974	1355	1282	304	100	337	605	694	1106	1883	589	1087	647	9989
1975	1182	483	1108	454	214	88	589	464	920	188	581	458	6729
1976	294	363	334	131	390	245	162	366	1135	1197	607	1098	6322
1977	819	1591	836	456	478	1372	148	1679	261	596	868	839	9943
Mean values for period 1844-1977													
Mean	Jan	Feb	Mar	Apr	May	June	July	Aug	Sept	Oct	Nov	Dec	Annual
S.D.	752	578	548	535	583	612	687	764	696	798	761	781	8095
Skewness*	0.10	0.78	0.98	0.54	1.12	0.76	0.35	0.67	0.63	0.68	1.09	0.68	0.33

\* Skewness = (Mean - Mode)/Standard deviation.

## Mesoscale surface humidity observations near the Home Counties tornado, 24 June 1979

By K. Grant

(Meteorological Office, Bracknell)

### Summary

The mesoscale surface humidity field at the time of the Home Counties tornado of 24 June 1979 is depicted in different ways. Some tornado predictors are examined. Comments are made on the usefulness of the United Kingdom climatological station network for 09 GMT synoptic investigations.

### Introduction

At about 0830 GMT on Sunday 24 June 1979 a tornado caused damage to a housing estate in Dedworth, western Windsor, Berkshire ( $51^{\circ}29'N$   $0^{\circ}39'W$ ), before moving north-north-eastwards on a heading of  $029^{\circ}$  across the racecourse, crossing the river Thames at Boveney Lock and causing further damage in the village of Eton Wick (Heighes 1979). Another report of wind damage came from King's Langley, Hertfordshire ( $51^{\circ}41'N$   $0^{\circ}27'W$ ) (Buller 1979).

The time of the tornado almost coincided with the observing time, 09 GMT, of United Kingdom climatological stations, when dry- and wet-bulb temperatures, present weather, wind and total cloud amount for more stations than at other times are recorded as well as elements referring to the previous 24 hours, so it was decided to plot the observations in different ways to see if any consistent pattern emerged, particularly in the humidity field. Other sources of data were hourly synoptic observations, atmospherics (SFLOC) reports, Meteosat pictures and routine upper-air reports.

### Synoptic context

From the *Daily Weather Report* it was apparent that the tornado formed in association with a trough in the polar maritime air behind a cold front. There was a vigorous westerly flow over southern England with an area of slack low pressure to the north. It was seen from satellite pictures that the cloud corresponded to the model of a comma-shaped mass around a positive vorticity centre to the rear of a cold front.

### Description of the 09 GMT surface synoptic chart

At 09 GMT the cold front is over the North Sea, while a small low-pressure area, centre 1004 mb, is seen to be positioned near Reading with the trough extending southwards (Figure 1). The low had been moving north-eastwards on a heading of  $060^{\circ}$  at about  $10 \text{ m s}^{-1}$  with little change of central pressure. Pressure tendencies are not large, the highest falling value being  $-2.2 \text{ mb/3 h}$  at Bracknell. An area of rain covers North Wales and the west and south-west Midlands; it is of moderate intensity over the Cotswolds to the north-west of the low. There is some light rain in Kent and Essex, corresponding to the 'tail of the comma' and showery precipitation in the south, both behind and ahead of the trough.

The tornado thus appeared about 40 km to the east of the low centre, in the forward right quadrant relative to the direction of motion. Thunderstorms were recorded at Bracknell (Beaufort Park), 15 km south-west of Windsor, between 0811 and 0835 GMT and at Hurley, 14 km west-north-west of Windsor, at 0815 and 0840 GMT. Surface winds were southerly ahead of the trough with a small area of south-east to easterly winds near the tornado. Dew-points were about  $9^{\circ}\text{C}$  behind the trough and  $11^{\circ}\text{C}$  ahead of it with the highest inland value  $12^{\circ}\text{C}$  at Heathrow.

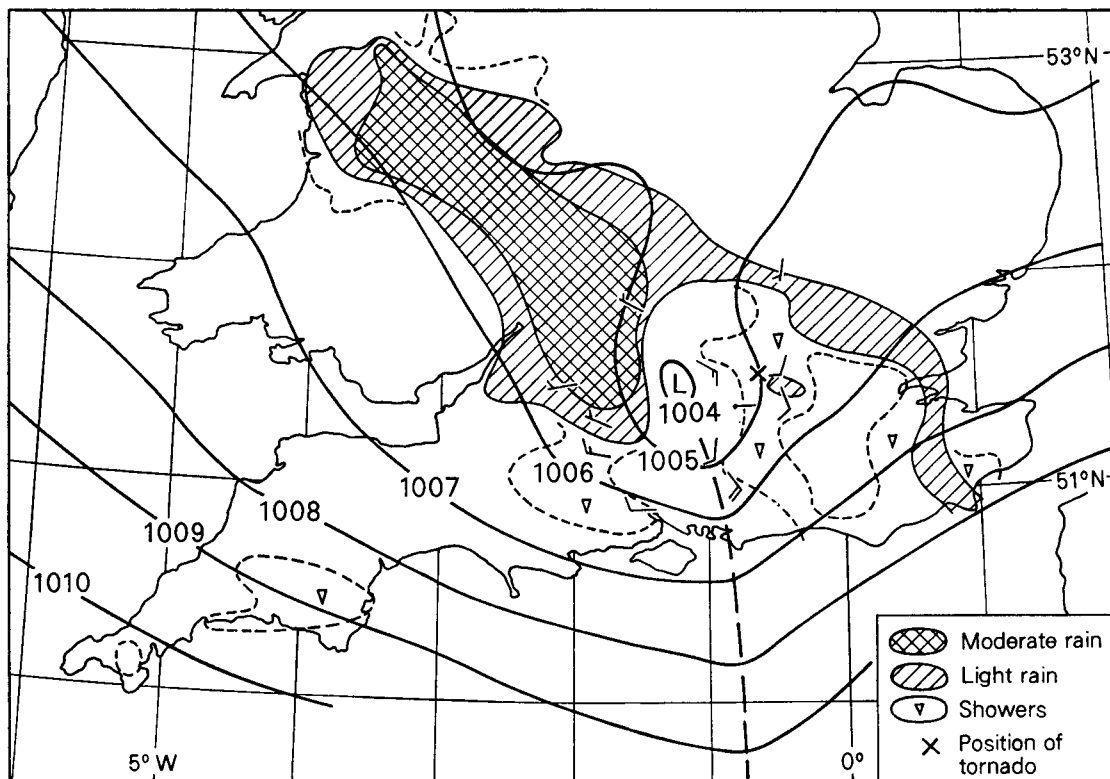


Figure 1. Mean-sea-level pressure and weather at 09 GMT on 24 June 1979.

### Humidity and cloud at 09 GMT

Detailed mesoscale maps of the screen-level humidity field can be produced using observations stored on magnetic disc from the climatological station network, and plotted either by hand or by computer on to microfilm, from which paper enlargements can be made. Because of the dense network, single 'obviously' wrong values can be ignored when drawing up the charts if they are unsupported by neighbouring values. Wind arrows were also plotted on the original maps.

Three different representations were examined:

(1) *Dew-point* (not shown). This was calculated and plotted to a precision of 0.1 °C, though 0.2 °C or even 0.5 °C would suffice. Isopleths at 1 °C intervals were found to be appropriate. The chart is the simplest of the three to plot and the most familiar to forecasters.

(2) *Wet-bulb temperature reduced to sea level* (Figure 2). This parameter was preferred to wet-bulb potential temperature since an atmospheric pressure value is not needed for its calculation. Only a small proportion of co-operating climatological stations report pressure, and since July 1978 pressure has not been transferred to the archived data set for these stations. A saturated adiabatic lapse rate of wet-bulb temperature is assumed; this varies with temperature, but in the present case a constant rate of 5 °C/km has been used for simplicity. Isopleths are drawn at 0.5 °C intervals. This chart is perhaps

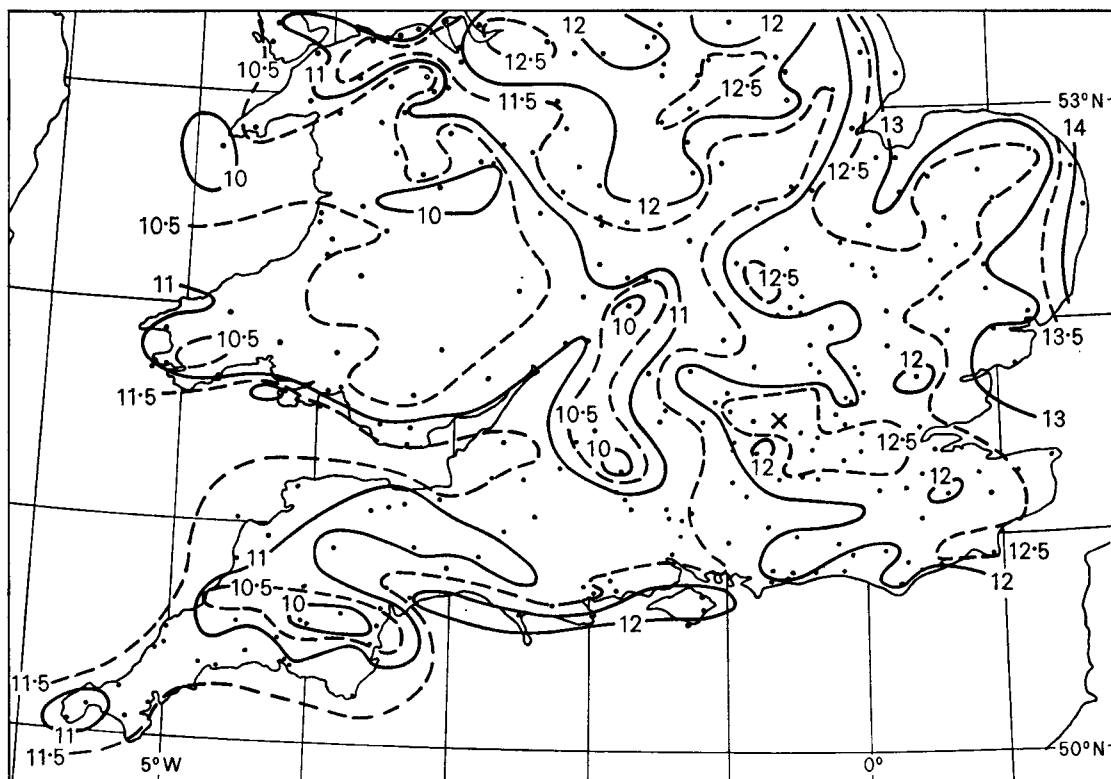


Figure 2. Wet-bulb temperature ( $^{\circ}\text{C}$ ) reduced to mean sea level at 09 GMT on 24 June 1979. The position of the tornado is marked by a cross. Dots indicate the stations used.

the best for representing variations of convective instability, assuming that upper-air temperatures change little in the mesoscale.

(3) *Wet-bulb temperature anomaly relative to June 1979 mean* (not shown). Here the mean 09 GMT wet-bulb temperature for 1–30 June 1979 at each station is subtracted from the value on the 24th. This is easily done by the computer, since all the values for a single month are stored together, and has the advantage that systematic thermometer and observer errors are mostly eliminated, though periodic or semi-random errors such as having different observers on different days of the week or occasional drying out of the muslin cannot be allowed for. A certain number of stations are lost here because of gaps in the record of more than a few days. The absolute values of the anomalies have little importance (on the 24th they were nearly all negative) but the pattern of wet-bulb temperature variation in the horizontal can be depicted with a large degree of confidence. Erroneous values are fewer and are easier to pick out. Anomalies at neighbouring stations often agree within  $0.1^{\circ}\text{C}$  or  $0.2^{\circ}\text{C}$  so that significant mesoscale differences of  $1\text{--}2^{\circ}\text{C}$  can easily be distinguished by  $0.5^{\circ}\text{C}$  isopleths.

The main features of the humidity field, as shown by all three maps, are (a) the moist air to the east and south-east of the low, (b) three tongues of less humid air to the west of the low, and (c) a rather irregular pattern to the north-east of the low, with higher humidity towards the cold front over the North Sea.

The tornado is situated near the centre of the most humid air, which stretches west-north-westwards from London as far as Oxford and appears to be associated with surface winds from the south-east quadrant. Highest dew-points are 12.2 °C at Hurley and 12.1 °C at Aldenham School (Hertfordshire), and Waddon and Bromley in south London. The highest wet-bulb temperature recorded in the area are 12.7 °C at Greenwich, 12.6 °C at Hurley and 12.5 °C at Heathrow, Wisley (Surrey), Waddon and Bromley.

The two centres of low humidity content to the north-west and south-west of the low are associated with the belt of continuous moderate rain. Further to the west, the cool polar maritime air is drier after passing over the Welsh mountains and Exmoor than after moving up the Bristol Channel.

To the north of London humidities are low where showers are breaking out ahead of the moist southerly winds.

Facsimile Meteosat pictures at half-hourly intervals were available for the period around 09 GMT, though resolution was poor. Dense white clouds were shown ahead of the low and behind it, but a dark patch was evident near or just to the east of its centre. This ties in with reports of 5 or 6 oktas of cloud from stations over the western Chiltern Hills, between Reading and Aylesbury. The rear edge of the cloud to both east and west was sharp, indicating cumulonimbus cloud.

The thunderstorm spawning the tornado did not give any SFLOC reports) these are coded hourly for the 10 minutes before the hour, so lightning activity may have either ceased by 0850, or been missed because of nearly simultaneous flashes from more active sources.

The highest dew-points ahead of the trough were at its northern end, to the east of the low. This moist air came from the south-west and may have had a longer sea track or a more southerly origin than the air to the south of it. The intrusion of the moist low-level air into a cold air mass would certainly cause potential instability, but the exact position of the tornado may depend on the detailed convergence and rotation of the low-level wind, which is not apparent from the surface winds at 09 GMT. The tornado occurred some 40 km ahead of the trough line, and winds away from the thunderstorm downdraught remained southerly for about an hour until the trough passed.

The topography may have had some influence on the tornado's formation. St Leonards Hill stands about 70 m above and to the south-south-west of the river with a fairly steep slope only 1 km upwind of the first grounding of the funnel cloud. The tendency for tornado formation in valleys or on the lee side of hills (Wright 1973) is therefore supported.

### **Upper-air features and thunderstorms**

The intense upper trough over the British Isles was the most noticeable feature, with a strong jet bounding it. The tropopause within the trough was near 400 mb and the 1000–500 mb thickness about 541 decageopotential metres, very low for the time of year. At 300 mb the 100 kn isotach moved around the base of the trough at about the time of the tornado, an occurrence known to be favourable for cyclonic development. The most intense high-level horizontal wind shear was probably south of the area of interest, over the English Channel.

Thunderstorms broke out at first near the trough in the Thames Valley, then more widely in the afternoon in Kent, East Anglia and parts of northern England. There were only a few reports of hail at climatological stations, all of the hail being less than 10 mm in diameter except for some of 20 mm diameter at Manston. An interesting point is that the thunderstorms associated with this trough (and with the preceding cold front) were the only ones to affect climatological stations in England in the 38-day period 16 June–23 July 1979.

### Examination of tornado predictors

The Americans have studied in detail the factors to be looked for in predicting tornadic activity. Alaka *et al.* (1973), studying surface predictors by regression methods, found that convergence of moisture at the surface was most relevant, though early in the day when surface winds were light or calm the best predictor was the horizontal gradient of moisture. The next most important parameters were convergence and vorticity (cyclonicity) of the surface wind, low pressure, and low stability of the atmosphere. Miller (1972), approaching the subject from a bench forecaster's viewpoint, emphasized moisture ridges, dry-air boundaries and low-level jets at 850 mb, dry tongues at 700 mb and various other parameters at 500 and 300 mb. (See also Crisp 1979.)

Examining these parameters qualitatively in the present case we see that:

(1) Moisture convergence is probably present just to the north of the main axis of moist air where winds are decreasing downstream. There is also convergence of moisture a little further west, near the centre of the low.

(2) The gradient of moisture at the surface is not particularly marked near the tornado, though it appears to be associated with a maximum absolute value. There is a drop of 2.5 °C in 60 km in reduced wet-bulb temperature near the low centre, but this is not unusually large.

(3) Surface wind confluence (and probably convergence) is marked on the trough-line, but not to the east of it, while cyclonic vorticity is present near the low.

(4) The pressure of 1005 mb is below average.

(5) The stability of the air is very low. Tropospheric wet-bulb potential temperatures are mainly in the range 8–10 °C compared with 12 °C at the surface ahead of the low (see Figure 3, Crawley 11 GMT ascent).

(6) A dry tongue of air at medium levels could have penetrated ahead of the trough, tying in with the cloudless slot shown by satellite. The Camborne radiosonde ascent at 02 GMT, just behind the trough, showed such air, with a dew-point depression of 17 °C at 787 mb (Figure 3).

(7) No low-level jet was found.

Figure 4 shows the 12 GMT 500 mb contours with 60 and 80 kn isotachs and plotted temperatures. The trough was moving east-north-east at 15 kn, so its position at 09 GMT would be about  $\frac{1}{4}$  degree of latitude to the west-south-west of that shown. Also plotted are values of the Severe Weather Threat (SWEAT) Index (Miller 1972), which uses 850 and 500 mb parameters. The highest values behind the cold front are 195 at Crawley and Hemsby, well below the 400 required for tornadic outbreaks or 300 for severe storms in the United States. The largest contribution to the 195 was the stability term involving the 'total totals' (sum of 850 mb temperature and dew-point minus twice the 500 mb temperature) which reached 55.5 at Hemsby, quite a high value. The high SWEAT indices over central Europe were caused by strong upper winds or high 850 mb dew-points, or both.

Work is still proceeding in the United States on producing objective techniques for forecasting thunderstorms and tornadoes, mainly using regression methods on predictors derived from operational numerical model output and climatology (Reap and Foster 1979).

Tornadogenesis on the scale of an individual severe thunderstorm has been considered by Lemon and Doswell (1979). They state that when a supercell is collapsing and the updraught is decreasing, a rear-flank rotating downdraught starts to form at high levels and extends down to the surface, giving tornadoes. The horizontal velocity of the high-level wind compared with that of the updraught, and the dryness of the air, are relevant parameters. In the present case the maximum wind is only about 55 kn at around 300 mb, though the north-west to south-east gradient is large; any downdraught probably originated much lower down.

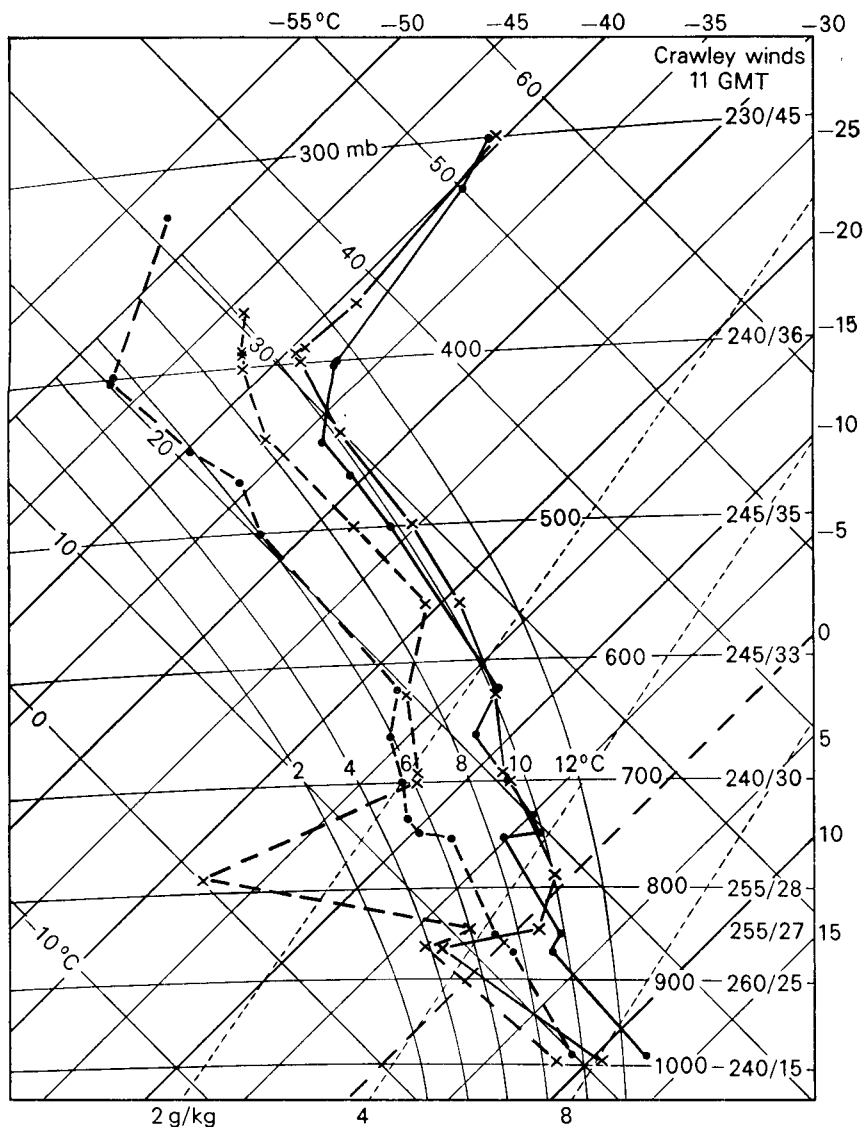


Figure 3. Tephigrams for 24 June 1979.

× — × (dry-bulb) and × - - - × (dew-point) Camborne 02 GMT  
 ● — ● (dry-bulb) and ● - - - ● (dew-point) Crawley 11 GMT

### Comments on the usefulness of the 09 GMT data set

The humidity maps discussed above show that mesoscale maps of screen-level humidity over the United Kingdom can be drawn for 09 GMT from routine observations (available a month or two in arrears). Figure 5 shows the frequencies of distance from a station reporting a wet-bulb temperature on 24 June 1979 to the nearest similar station, for England, Wales and Scotland combined. A random distribution of the same number of stations over a square area of the same size would give distances





*Photograph by courtesy of J. W. F. Russell*

Plate I. Tornado at Windsor, Berkshire, 24 June 1979. (See page 259.)



*Photograph by courtesy of Evening Mail Ltd, Uxbridge*

**Plate II.** Tornado damage to cowshed at Little Common Farm, Windsor.



*Photograph by courtesy of Evening Mail Ltd, Uxbridge*

Plate III. Repairing a roof damaged by the tornado.



*Photograph by courtesy of Evening Mail Ltd, Uxbridge*

Plate IV. Tornado damage at Windsor.

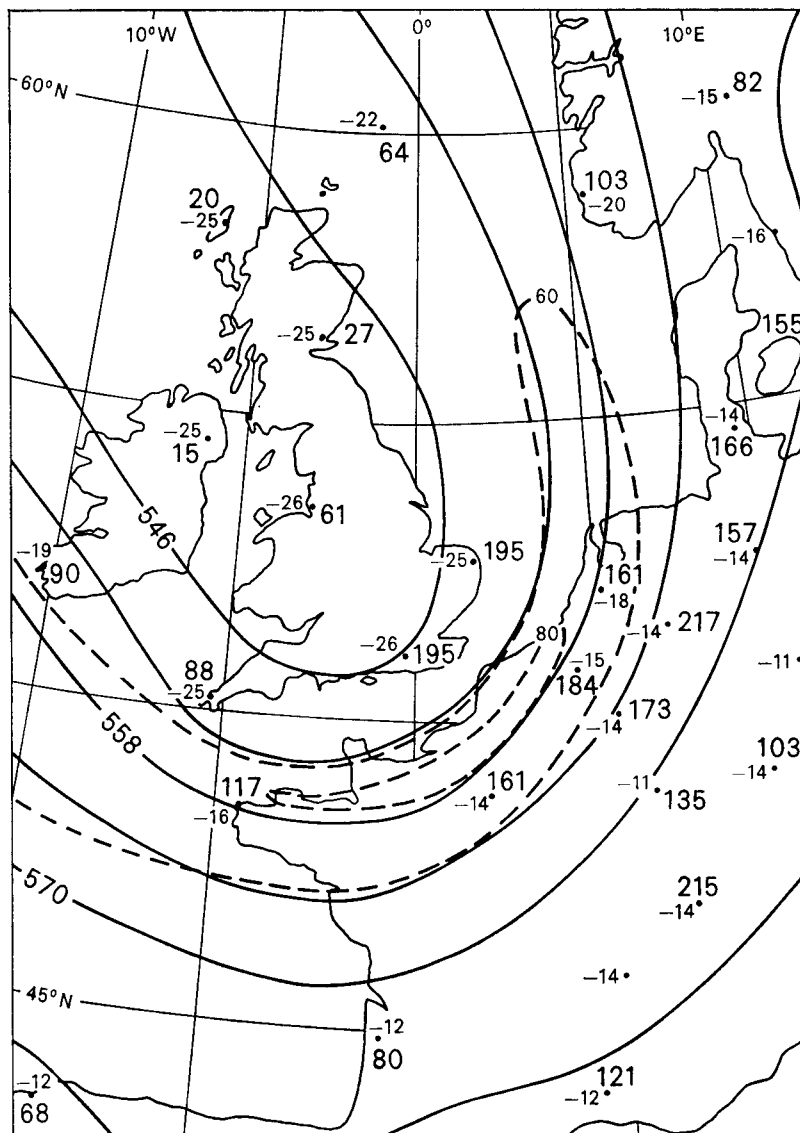


Figure 4. 500 mb chart for 12 GMT on 24 June 1979. Temperatures (°C) and SWEAT indices are also plotted. Continuous isopleths are labelled in decageopotential metres. Dashed isotachs indicate wind speed in knots.

shown by the dashed line, while equal spacing on a triangular grid would give a uniform distance apart of 23 km, neglecting edge effects, from the formula:

$$\text{distance} = (\sqrt{3}/2)^{-1/2} (\text{area/number of stations})^{1/2}$$

The actual network is better (because of the reduced number of small spacings) than for random spacing, but has not nearly the desirable spacing given by a uniform grid of points.

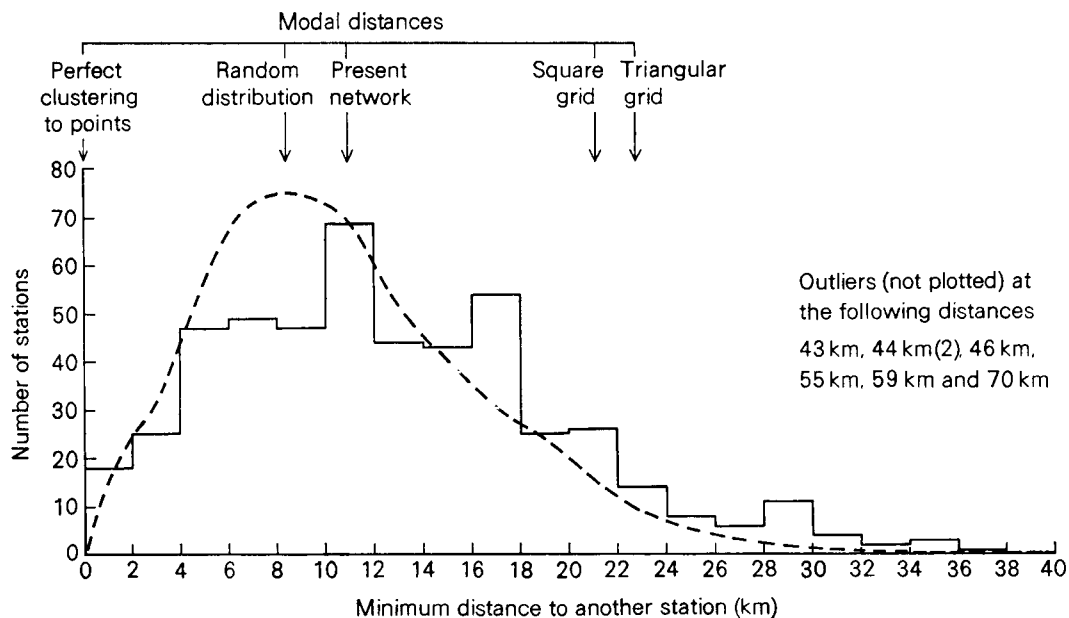


Figure 5. Spacing of climatological stations. The bar chart refers to 504 stations in England, Wales and Scotland reporting wet-bulb temperatures at 09 GMT on 24 June 1979 and the curve represents the spacing of 504 stations randomly distributed in a square of the same area (Monte Carlo simulation).

The 09 GMT data are stored on magnetic disc or tape at the Meteorological Office, Bracknell, for the years 1972 onwards. Synoptic data for the most recent years (temperature and dew-point to the nearest whole degree at present) are available for every hour of the day with average spacing of about 42–62 km, depending on the hour concerned (assuming a triangular grid). Also available are climatological hourly or fixed-hourly observations which give temperatures to the nearest 0.1 °C at an average spacing of 54–72 km, depending on hour of the day.

In quiet situations in summer the 09 GMT humidity chart may be relevant to afternoon or evening convection, but often the 12 or 15 GMT chart of synoptic stations (about 150 with dew-points, spacing about 43 km) will have to be used in addition. Atkinson (1977) used 09 GMT vapour pressures from climatological stations to investigate the Hampstead Storm.

For the whole of the United Kingdom on the particular Sunday in question the 09 GMT data set contained 495 reports of surface (10 m) wind, 476 of present-weather code figure, 527 of total cloud amount, 491 of visibility (coarse single-figure code at co-operating stations), 569 of dry-bulb temperature, 567 of wet-bulb temperature (from which dew-point, vapour pressure and relative humidity are derived) and 485 of state of ground. Similar 'daily' data sets record maximum, minimum and grass minimum temperature, rainfall, snow depth and days of snow, hail, thunder, gale, etc. Though the number of stations involved (Ogden 1978) is not as great as for rainfall, in case-study research the observations can delineate tongues of moist and dry air early in the day with an accuracy which synoptic observations cannot quite achieve. Inaccuracy and unrepresentativeness are more frequent than in synoptic data, but can be partially eliminated by the anomaly method or by more sophisticated quality control.

One important source of possible error in analysing case studies is the non-simultaneity of the 09 GMT observations. Once the observer agrees to make observations at 09 GMT there is no way of indicating to the user of the observation whether it was made late on a particular day, or whether indeed it is done regularly at times up to a half hour different from 0900. To position troughs etc. accurately on the mesoscale charts the time of the observation must be known to an accuracy corresponding to the time taken by a fast-moving feature in moving between one station and the next one downstream. For features moving at  $10 \text{ m s}^{-1}$  this is of the order of 15–30 minutes, and for  $30 \text{ m s}^{-1}$  only 5–10 minutes. Thus the resolving power due to the close spacing of the stations is negated for fast-moving situations if the timing is not known accurately. Most stations report very near to 0900 GMT but the investigator does not know which these are.

## Conclusions

The tornado occurred when the troposphere was extremely unstable. Upper winds overhead were not strong enough for American severe-weather indices to indicate severe storms or tornadoes. The factors which may have combined to set off this tornado were the extra potential instability above the most humid surface air, the dry air intrusion ahead of the trough, the increased vorticity caused by the approach of the small low-pressure area and possibly the local topography. Mesoscale climatological observations were useful in defining the exact extent of the moist air at the surface.

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- |  |      |   |
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## **The use of analysis of variance in the assessment of rainfall variability**

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### **Summary**

Analysis of variance has been used by other workers to assess the significance of differences between the catch of two or more rain-gauges. It is suggested that analysis of variance may be an inappropriate technique for this purpose, in view of the very large 'within-cell' variations that can arise.

### **Introduction**

Analysis of variance is a statistical technique that may be used to test the significance of effects on a given phenomenon. In the context of rainfall studies it is possible to use the technique to test whether the rainfall at one gauge or more than one gauge is significantly different from that at other gauges. The gauges so selected can be placed in classificatory groups (for example altitude bands) so that it is the effect of the external factor (for example altitude) that is being assessed. Reported use of the technique is not common and it is the purpose of this paper to point out that there are serious problems in its application to rainfall data, which need to be more fully evaluated before application becomes more widespread.

The use of the technique raises two separate issues:

(a) whether the results using analysis of variance reflect the nature of reality with adequate sensitivity, and

(b) whether the technique of analysis of variance can, in principle, be applied to rainfall measurement, given the statistical requirements of the test.

The latter question is a technical one which it is relatively simple to assess. The former, however, raises problems which are more complex.

### **Previous use of the technique**

Two recent papers have been published which both refer to the analysis of rainfall data from the Plynlimon instrumented catchment of the Institute of Hydrology (Clarke, Leese and Newson (1975) and Newson and Clarke (1976)). It should be stressed at the outset that the points made in the present paper do not invalidate the findings of either of these studies but merely indicate that there may be further significant variations which may have been inadvertently ignored.

Both the papers referred to use rainfall data collected on a monthly basis. In the first, Clarke, Leese and Newson (1975) evaluated the relationship between rainfall totals and altitude bands, slope and aspects. They concluded, in general terms, that altitude had a significant effect on rainfall but that slope and aspect had no effect. The second paper (Newson and Clarke 1976) compares the catch of ground-level and tree-canopy-level gauges. In this, they conclude that there is no significant difference between the catches of the two groups of gauges.

It was this latter finding that initially raised doubts as to the appropriateness of the statistical technique employed. The finding, that the null hypothesis of no difference between ground-level and

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canopy-level gauges could not be disproved, seems to conflict to some degree with those of most other workers in the field.

Reynolds and Leyton (1963) for example reported an unpublished comment by Law to the effect that an unshielded canopy-level or mast-top gauge at Stocks Reservoir, Yorkshire, recorded about 15 per cent less than a standard ground-level gauge at an equivalent site. In their own work Reynolds and Leyton reported that canopy-level gauges which were carefully 'nested' in the canopy (that is to say placed as low as possible in the canopy to obtain shelter from the wind, yet not so low as to be affected by interception or splash) recorded an average 2 per cent lower than standard ground-level gauges. Canopy-level gauges equipped with shields of various types recorded about 1 per cent less than their ground-level equivalent. In the work by Clarkson (1973) it was reported that a mast-top gauge under-recorded by 'less than 2 per cent' over a two-year trial period. All the canopy-level gauges used in the analysis by Newson and Clarke (1976) were 'nested' in a similar way to those of Reynolds and Leyton (Newson, personal communication).

### An example of the application of analysis of variance

In connection with a study of rainfall interception by coniferous forest, rainfall was measured in four rain-gauges at sites at Macclesfield Forest, Cheshire. Measurements were taken on a weekly basis and periods which included snowfall were excluded from the analysis. The field area of the study covered part of the western flanks of the Pennines, ranging in altitude from 260 m to 365 m. Three of the gauges were installed with their apertures at ground level and were surrounded by baffle-plates, while the fourth was installed at canopy level and 'nested' as described in the previous paragraph. Over a 78-week period the total rainfall measured was as shown in Table I.

**Table I.** *Rainfall totals for the four rain-gauges analysed at Macclesfield Forest.*

Rain-gauge	Type	Altitude (m)	Rainfall over 78-week period (mm)
A	Ground-level	260	1338.5
B	Canopy-level	345	1357.3
C	Ground-level	335	1462.1
D	Ground-level	365	1383.8

One-way analysis of variance was used to assess the variance between pairs of gauges in turn:

(a) A and D (i.e. 'low-altitude' and 'high-altitude'), and

(b) B and C (i.e. canopy-level and ground-level).

The one-way analysis of variance procedure follows that described by Snedecor and Cochran (1967). Briefly, it involves the calculation of the *F*-ratio from:

$$F = \frac{SS_A/(k-1)}{SS_{\text{error}}/(N-k)},$$

where

$$SS_A = \sum_j N_j (\bar{Y}_j - \bar{Y})^2 \text{ (the sum of the squares 'between sites')}$$

in which  $\bar{Y}_j$  is the mean of the variable *Y* in the category *j* and  $N_j$  is the number of cases in the category *j*, and

$$SS_{\text{error}} = \sum_j \sum_i (Y_{ji} - \bar{Y}_j)^2 \text{ (the sum of squares 'within sites'),}$$

and

$$k = \text{number of sites.}$$

Analysis of variance is a parametric technique in which it is required that the data should be normally distributed, although this requirement is not absolute and can be relaxed to a certain degree. Nevertheless, since the weekly data were negatively skewed, they were first subjected to a logarithmic transformation. (Clarke *et al.* did not employ any transformation on their raw data (Clarke, personal communication), although it is important to note that they used monthly data in their study and the distribution of these data would tend to approach the normal more closely than weekly data.)

A further requirement for analysis of variance is that the variables are statistically independent. This requirement is not, however, absolute and it is clear that the longer each rainfall total period is, the greater likelihood there is that this requirement is satisfied. Hence, for example, daily rainfall totals would grossly violate both this requirement and that for normality. On the other hand, monthly totals would be unlikely to violate either. The weekly data given in this example having been subjected to a logarithmic transformation, it is most unlikely that either of these requirements is violated to a significant extent.

The results from the analysis of variance in the present study are given in Table II. Comparisons of the calculated *F*-ratio with the *F*-distribution show that there was no significant difference in the case of either of the the tested pairs of data sets (a) and (b) in the Table.

**Table II.** *Results of the analysis of variance of Macclesfield Forest rainfall data*

Data set (a): Rainfall at A v. rainfall at D

74 weekly records

Source	Degrees of freedom	Sum of squares	Mean sum of squares
Between sites	1	0.8936	0.8936
Within sites	146	479900.3996	328.0849
Total	147	479901.2932	

*F*-ratio = 0.0027 (not significant at  $p = 0.05$ )

Data set (b): Rainfall at B v. rainfall at C

75 weekly records

Source	Degrees of freedom	Sum of squares	Mean sum of squares
Between sites	1	90.6371	90.6371
Within sites	148	48428.8299	327.2218
Total	149	48519.4669	

*F*-ratio = 0.2750 (not significant at  $p = 0.05$ )

(It should be noted that in simple two-variable cases, such as those presented here, it is possible to use a *t*-test to establish whether the means of the records from the two rain-gauges are significantly different: in both cases (a) and (b) the *t*-test also demonstrated that there was no significant difference at the  $p = 0.05$  level.)

## Discussion and conclusions

The conclusion that analysis of variance showed no difference between the sites was initially surprising, particularly in view of the comparison of rainfall totals (Table I). However, examination of the data suggests that the inherent variability of the rainfall data acts as a major source of variation in the analysis. This variation is picked up in the within-cell variability and will therefore tend to reduce the significance of the between-cell variability. In Clarke's cases the (within-cell) rainfall variability will tend to be less than in the Macclesfield Forest example because data for his work were analysed on a monthly basis. (As already noted, this will also have the effect of reducing the skewness of the data.)

Any assessment of the efficiency of analysis of variance partly hinges on the definition of 'significant', not only in a statistical sense but in its wider use. Hence, for example, Reynolds and Leyton (1963) seemed to indicate that a 2 per cent deficiency in catch should be regarded as significant, whilst Clarkson (1973) implied that the 'less than 2 per cent' deficiency he noted was insignificant. The decision as to what is significant obviously depends partly on the error levels elsewhere in the study, and, in the context of the Macclesfield Forest study quoted, a 2 per cent error would be considered insignificant.

A further argument against the use of analysis of variance (and indeed of most relatively sophisticated statistical techniques) with rainfall data is that it involves the artificial delineation of units which are analysed as if they were naturally discrete in time. In reality, rainfall is more akin to a continuously changing variable over time.

To return to the two aspects of the problem raised in the introduction to this paper it is clear that, firstly, the use of analysis of variance imposes an implied structure on the reality of rainfall data which may not reflect reality with sufficient sensitivity. Secondly, although the technique can validly be applied with care to rainfall data there are pitfalls which must be avoided if the results are not to be ambiguous. In particular its use with short-period data (less than month-long) raises special difficulties.

### Acknowledgements

Although the opinions in this paper are entirely those of the author he would like to thank Mr R. T. Clarke and Anna Newson (Institute of Hydrology) for providing helpful additional information. Thanks are also due to Dr S. Nortcliff (University of Reading) for helpful discussion and to the North West Water Authority for co-operation in the collection of field data.

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## An invitation to form a meteorological system of observations by world-wide agreement

By James Jurin, M.D.

Secretary of the Royal Society, Fellow of the College of Medicine, London  
(London, Royal Society, *Philosophical Transactions*, 32, 1722, 422-427)  
*Translated from the Latin by Dr J. G. Landels, University of Reading*

The various conditions of the sky, and of the air which we breathe, that is to say the changes and alterations of cold and heat, of dryness and humidity, particularly those which are great and sudden, are rightly considered to be factors in the health of the human race. Not only Doctors, but also men of all ages (past and present) who have been keenly interested in Natural Philosophy have asserted that effort and work spent in observing these phenomena is not to be despised. In the last century, instruments and mechanisms have been devised by the ingenuity and effort of Philosophers, by means of which movements and changes in the weight, heat, humidity and (*elateris*)\* of the air can be instantly presented to view, and at the same time submitted to weighing and measurement of a very delicate and accurate kind.

And these distinguished men did not see fit to stop there, but, driven by zest for study and desire for knowledge, strove to investigate the causes of these changes, where it was possible. To this end they made careful notes in their diaries of the readings taken from the recently invented instruments of the weight, humidity and temperatures of the ambient (air), and added observations on the appearance of the sky and the weather, the winds and the amount of rain; these notes can be found scattered about in the *Acta Philosophica* and elsewhere.

It would be difficult to find a better method or system of observation than that. But just suppose there were observers in suitable numbers, appropriately distributed over a large area of the earth's surface; and, ultimately, someone to collate their various diaries, and make notes of the agreements and discrepancies; we should then before long have a Meteorological History covering a number of years, of a kind that could hardly be imagined or even dreamed about today.

We find that there is general acceptance of the view that sudden changes in the weather are to be attributed to the wind; and when it became possible, by the system of observations described above, to discover in what regions they originate, what course they follow, at what times (or for how long) and over how great an area of the world's surface, with this knowledge the way might be opened to an understanding of the causes and origin of winds. At any rate this theory (i.e. that winds cause changes in the weather) which has a very important bearing on our present subject, and which is generally regarded as a reasonable hypothesis, we would be able to prove either true or false by reliable observations. I quote the opinion of that most learned of men, Edmund Halley (*Philos Trans R Soc* No. 181) that the mercury rises in the barometer because the winds, blowing on either side from opposite points thicken the air and, as it were, pile it into a heap; and on the contrary the mercury falls because the air is carried away from that place by winds blowing in diverse directions, and, as it were, is drained away.

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\* *elateris* in the original is a misprint; the context suggests 'wind force or direction' (perhaps *flationis* or *flatilis*? or *lateris*, of the quarter).

Educated people, therefore, who are willing to contribute some effort towards the perfection of this branch of Natural History are asked to note in a diary, at least once every day, or oftener whenever possible, the height of the barometer and thermometer, the wind direction, with an estimation of its force, the state of the sky and the amounts of rain and snow which have fallen since the previous observation; and should anyone wish to add observations of the hygroscope, or observations made with the aid of the magnetic needle, these would not be unwelcome.

Should a violent storm befall it would be profitable to have an accurate plotting of its beginning, growth, peak of violence, decay and ending, accurately recorded together with notes of the times and heights of the barometer corresponding to these times.

We advise those who are skilled enough to make and fill a barometer themselves to use the common or so-called open barometer. The tube should be at least one-quarter or even as much as one-third of an inch wide, because in a narrower tube the mercury has been found to subside below the true height (*Philos Trans* No. 363). The diameter of the cistern, or mercury reservoir, should be at least eight or ten times that of the tube, in order that whether the mercury ascends or descends in the tube its level in the cistern will remain constant, or at least change very little.

Those, indeed, who prefer to use the closed, or portable barometer may obtain such a one, made with the greatest diligence, from that notable instrument maker Francis Hawksbee, in the area commonly known as Crane Court, London, who also will provide thermometers marked with that scale, or graduation marks, engraved with particular care,\* which he has already made familiar to men of science for many years.

Those, however, who use a differently constructed thermometer are asked to be so kind as to note in the diary the site of the thermometer, its construction, arrangement of degrees on the scale, and the maker's name from whose workshop it came. We judge the most suitable site for a thermometer to be in a room facing north, where a fire is never, or at least very rarely, lit.

So that the diaries may the more easily be compared it is desirable for them all to be in the following form:

The first column should show the day and hour of the observation; we particularly ask that observers use the old, or Julian, style; the second should show the height reached by the mercury in the barometer above the level of the mercury in the reservoir in inches or twelfth† parts of the London foot, and tenths parts thereof. The London foot is to the Paris foot as 15 is to 16 approximately; the third column should show the degrees and tenth parts which the spirit reaches in the thermometer, the fourth should represent the direction and force of the wind, the force always being represented by one of the sequence of numbers 1, 2, 3, 4 of which 1 signifies the most gentle motion of the air, scarcely stirring the leaves of trees, 4 the very peak of a violent wind, 2 and 3 intermediate between these limits and zero or 0 denoting a flat calm, the fifth should be occupied by the state of the sky and a brief description of the weather, and the sixth and last should show the amount of rain and melted snow which has fallen since the previous observation measured in London inches and tenths. This may easily be estimated by means of a funnel about two or three feet in diameter, a container for receiving the water which flows down the funnel, and a measuring cylinder with a scale divided into inches and tenths.

The funnel should be sited so that whichever way the wind blows no part of the rain shall be intercepted by any building or any other obstruction. The container for holding the rainwater should be everywhere well closed to prevent evaporation, save for one narrow hole to receive the rainwater from

---

\* This could mean that the scale is well engraved, or that it is a particular scale.

† The author here uses *digitus* which is normally understood to mean  $\frac{1}{16}$  inch, i.e. one-sixteenth part of a foot.

the funnel above. The diameter of the measuring cylinder should be a tenth part of the diameter of the funnel so that one inch of water in the measure represents a hundredth part of one inch falling into the funnel and therefore on the surrounding area, and similarly for each decimal part of an inch.

At the end of each month and each year the monthly and annual mean heights should be reckoned, of the barometer and the thermometer, and also the total amounts of rain for each month and each complete year. The mean heights can be calculated by adding together all the barometer heights, also the thermometer readings, either the morning readings or the daily maxima (which of course occur about the third or fourth hour of the afternoon), and dividing that sum by the number of days.

We ask all who are willing to undertake all, or any, of the above observations to send copies of their diaries covering each year to the Secretaries of the Royal Society so that they may be gathered together in one diary which will be prepared in London by the order of the Royal Society. It is planned that whatever can be deduced from the collation of these diaries will be communicated to the public each year in *Acta Philosophica*.

#### Form of Diary

Day & Hour 1723	Barom. height inch tenth	Therm. height deg tenth	Wind	Weather	Rain inch tenth
November					
1. 8 a.m.	29.75	49.6	S.W. 1	sky covered by clouds	0.035
4 p.m.	29.56	47.3	S.W. 2	intermittent rain with sun in between	0.043
2. 7 a.m.	29.24	48.5	S. 1	almost continuous rain	0.725
3. 9 a.m.	29.95	49.7	N. 1	clear sky	0.032
5 p.m.	30.4	49.2	N. 1	clear sky	0.000
4. 7 a.m.	29.9	47.0	S.W. 1	scattered clouds	0.000
10	29.7	46.2	S.W. 2	intermittent rain	0.103
12	29.4	45.0	S. 3	sky almost everywhere covered with clouds	0.050
3 p.m.	28.8	46.0	S. 4	scattered clouds	0.000
5	28.6	47.2	S.W. 4	sky unchanged	0.000
7	28.9	48.0	S.W. 2	it rained	0.000
9	28.9	48.2	0	almost continuous rain	0.305
5. 7 a.m.	29.7	53.4	N.E. 1	clear sky, frost	0.250

## Reviews

*The atmosphere: endangered and endangering*, W. W. Kellogg and Margaret Mead (Scientific Editors). 230 mm × 155 mm, pp. xii + 154, illus. Castle House Publications, Tunbridge Wells, Kent, 1980. Price £6.

The title of this book is of course fatuous: the atmosphere is not endangered. Indeed, in an idle moment readers might like to imagine a means of accomplishing this proposition, and thereby come to a better appreciation of its improbability. As to the atmosphere being endangering, this is a prospect which arises mainly from it acting as a transporting medium for the manifold molecules emitted by the activities of the current human population; any reasonable cost-benefit analysis would still show the atmosphere to be undeserving of a ban by the various environmental protection agencies, however.

The vagaries of the process of title selection can be, one supposes, squarely blamed on the publishers, who also deserve a brickbat for publishing in 1980 the proceedings of a conference which took place over four years ago, a fact which can only be deduced indirectly from the book; however, the scientific editors must take responsibility for producing a text which is an uneven mixture of scientific and social commentaries.

The Preface was written by Margaret Mead; and while one hesitates to criticize this late social scientist of outstanding stature, a judgement which equates extreme caution among scientists to 'fiddling while Rome burns or dancing on the eve of the Battle of Waterloo' cannot be supported by experience in atmospheric science. This issue goes to the heart of the current environmental debate, particularly as far as the global aspects are concerned. The Preface as a whole calls for scientists to connect science effectively to political decision-making on a global scale, without however explicitly acknowledging the realities of a world of nation states. It is not obvious that global problems of the atmosphere cannot be solved within the present system; if they cannot, one is tempted to observe that they won't be.

*Part 1: Summary and recommendations.* This is yet another consideration of the now familiar catalogue of potentially large-scale pollution episodes: aircraft, nuclear weapons, fertilizers, chlorofluorocarbons, carbon dioxide, sulphur dioxide and radioactive gases ( $^{85}\text{Kr}$  and  $^3\text{H}$ ). At this stage, what matters most is not exactly what numbers are assigned to the effects of the various gases, but the way in which the inadequacies of the calculations are discussed. We are told that to ignore the possibilities of such changes is, in effect, a decision not to act. Such an assertion ignores the fact that one of the most uncertain things about these potential global pollution episodes is their time-scale, and that present models cannot predict the natural fluctuations of the quantities concerned; thus even if doubled  $\text{CO}_2$  does increase surface temperatures in the models, and the effect does operate in the real atmosphere as calculated, there is no certainty that it will not be lost in the natural fluctuations. If long-range planning on a time-scale of decades is to be undertaken, then the concept of predicted effects being unequivocally above the level of natural variability should see more use than is apparent in this book.

In *Part 2: The atmosphere and its climate*, this difficulty is again returned to: one school of thought holds that calculations which predict 'great societal risk' should be acted upon, even if the calculations are incomplete, because there is no way of dismissing the possibility that the calculated effect will prevail. On the other hand, the position as stated by Smagorinsky 'If current physically comprehensive models are inadequate to answer some of our questions, then certainly we should be wary of basing broad national or international decisions on hand-waving arguments or back-of-the-envelope calculations' is characterized as extremely conservative. While your reviewer would replace 'hand-waving arguments or back-of-the-envelope calculations' by 'incomplete models'—and note that in his view there is no such thing as a 'current physically comprehensive model'—he sides with this second view.

*Part 3: Human costs and benefits of environmental change* again covers now familiar ground, with

human usage and supply of energy and food considered from varying points of view: scientific, economic and ecological. The analyses are brief, and usually argued by an individual participating in the conference; these authors are able to recommend the adoption of various strategies for future food and energy supply.

*Part 4* summarizes the first day's discussion, and is a series of paragraphs describing the views of a succession of people identified mainly as 'a participant', 'one scientist', 'another scientist', 'a conferee', etc. The chapter conveys a good feeling of the quality and coherence of the discussion at a typical scientific conference.

*Part 5: Managing the atmospheric resource: Will mankind behave rationally?* is mainly about neither management nor behaviour, but is rather concerned largely with the way international law and organizations, such as the UN agencies, impinge upon national governments. To an atmospheric scientist, much of the argument seems hypothetical; the sprinkling of coal dust over the Arctic and Greenland ice caps, and large-scale weather modifications are one feels projects which have a long and hazardous road to travel prior to becoming actualities. The reader is also treated to the revelation that national governments react to international problems on the basis of maximizing benefit to themselves; this seems to be the main message, although it is obscured by the use of jargon like 'increasing and decreasing sum games', portentous statements of the obvious and startlingly confident expectations.

*Part 6: The atmosphere and society* is a position paper written before the conference by Kellogg, and defines the areas of interest for the participants: they are largely as listed in Part 1, with the addition of land-use changes, particle inputs and the addition of heat.

There are five appendices, dealing with (i) The carbon cycle and the palaeoclimatic record, (ii) The interaction of the atmosphere and biosphere, (iii) Some thoughts on control of aerospace, (iv) International structures for atmospheric problems, and (v) Some comments on technology and the atmosphere. Their fancifulness increases in the same order, and they form a suitably irritating end to an annoying book.

Throughout the book there are sections recording the arguments which took place at the conference, and although they are almost inevitably patchy, were nonetheless the most interesting passages, if only for seeing who said what about which topic.

This book has added yet another to the growing list of proceedings of international conferences on environmental matters which have been published in the last decade. It is a good deal worse than most of its predecessors, which in themselves were no great monuments to scientific insight or literary skill. The most effective course of action will be publication of the scientific work in journals, with national governments taking the best scientific advice available to them and acting as they see fit. Overblown jargon-laden statements about the need for international decision making, based on inadequately characterized atmospheric 'threats', are of no practical utility; this vapid volume abounds in them, and it cannot be recommended.

A. F. Tuck

*Chlorofluorocarbons in the environment: the aerosol controversy*, edited by T. M. Sugden and T. F. West. 235 mm × 155 mm, pp. 183, illus. Ellis Horwood Ltd, Chichester, Sussex, 1980. Price £17.50.

Having waxed unenthusiastic about the proceedings of a conference being published in book form in the previous review, it is a pleasure to be able to reverse attitude and recommend this volume. The crucial difference appears to be that selected, authoritative speakers were given a brief to produce a publishable paper at the meeting, and present it to a critical and informed audience. This advance



focusing effect, and the highly specific choice of subject, appears to have prevented the incoherent vagueness of the previous book. The conference took place in October 1978, and was set up by the Society of Chemical Industry.

*Chapter 1, Background and present position* by T. M. Sugden of Cambridge University, is a short survey of the background and position at the time of the conference. It is reasonable and balanced, laying some emphasis on the need for total risk analysis and the uncertainties in model results. It is slightly marred by the statement that the recovery time for stratospheric ozone from an all-out nuclear war would be about a century; this is not so, because the time-scale is the five years or so imposed by intra-stratospheric mixing rather than the decades required to cycle the entire atmospheric mass through the photochemically active regions of the stratosphere.

*Chapter 2, The chlorofluorocarbon/ozone issue: the industrial view*, is by G. Diprose of ICI Ltd, and gives an account of how the non-Eastern-bloc countries have organized a research program, through the then Manufacturing Chemists Association. The point is made that a program was organized in 1972, two years before the ozone depletion issue was raised by Rowland and Molina, to determine the ultimate fate of fluorocarbons in the environment. The long time-scale needed to build and amend the multiplant manufacturing process for these molecules, and their ideality for the uses to which they are put, are also pointed out. The research program is funded at \$1.4-2.0 million per year; the largest and most important work is that of the atmospheric lifetime experiment, where four or five stations have been set up on remote islands (distributed between both hemispheres) to measure as accurately as possible the trends in the atmospheric fluorocarbon mixing ratios, with the objective of thereby determining whether or not the actual lifetimes are in accord with those calculated by the models. A wide range of other activities is funded; one of these which has impinged on meteorologists is the conclusion drawn by statisticians experienced in the chemical industry, that total ozone measurements by the Dobson network are a better detector of long-term trends than many scientists associated with the instruments are willing to admit. This remains controversial. Finally, the expense and difficulty of finding suitable alternatives to fluorocarbons is presented.

*Chapter 3, Stratospheric chemistry* by B. A. Thrush of Cambridge University is a concise review of the chemical kinetics underlying the stratospheric ozone balance. He states a preference for the lower amounts of ClO measured in the upper stratosphere by Waters and by Anderson, rather than the inexplicably high profiles measured on two occasions by the latter. This view tends to be supported by Anderson's most recent data, but not by one set of measurements by Menzies in the upper stratosphere. As the author points out, a sufficient volume of data to establish a reasonable average is not available. After drawing attention to the cautionary tale of the predicted effects of supersonic transport aircraft, he further points out the inconsistency between theory and experiment in the matter of stratospheric concentrations of pernitric acid ( $\text{HO}_2\text{NO}_2$ ) and nitrogen pentoxide ( $\text{N}_2\text{O}_5$ ). After emphasizing the importance of uncertainty limits, the author expects these to be narrowed by continuing research, but with no dramatic changes in present understanding of the chemistry.

These papers are followed by a report of a discussion between named participants.

*Chapter 4, Halocarbons in the atmosphere* by J. E. Lovelock and P. G. Simmonds of Reading University is an account of the measurements made by the first of these authors, occasionally with collaborators, over the course of the 1970s. In a provocative chapter, issue is taken with those who criticized his first measurements of the interhemispheric ratio of  $\text{CFCl}_3$  in 1971/72, and a convincing argument mounted that a true north-south ratio, at least over the Atlantic, needs a long time-series of measurements. From his position as inventor of the electron capture gas chromatograph, Lovelock points out the uncertainties in current measurements, which do not preclude the existence of significant tropospheric sinks for chlorofluorocarbons. He also points out the variability and relatively high abundance

of naturally occurring methyl chloride, and even proposes it as a possible atmospheric source of carbon tetrachloride, a molecule considered in other published work to be almost wholly of industrial origin. The matter is unresolved, but the points made are telling and require resolution.

*Chapter 5, Chlorofluorocarbons in the atmosphere: the meteorological problems* by R. J. Murgatroyd of the Meteorological Office is an examination of the relevant meteorological processes, and what is involved in their mathematical representation. Initially, it is stated qualitatively how the stratospheric composition, the radiation and motion fields are interactively coupled, an elementary point which, however, is still not represented in model calculations with sufficient thoroughness. The mean temperature and wind structure is described and interpreted, and followed by an account of the eddy motions of various sorts (sudden warmings, long waves, synoptic-scale features, etc.). The consequences of these motions for zonal mean modelling efforts are pointed out, and some results presented for the calculated transport of chlorofluorocarbons on a global scale. The conclusion is drawn that a better basis needs to be derived for one- and two-dimensional models, and that this will demand the preparation and analysis of adequate three-dimensional models.

*Chapter 6, Modelling stratospheric motions and their influence on ozone* by J. A. Pyle and J. T. Houghton of Oxford University, uses the two-dimensional zonal mean model developed in their department as a vehicle for discussion of the modelling of stratospheric ozone. Despite its ability to give reasonably realistic simulations, detailed considerations by the authors of the model reveals inadequacies in its representation of the large-scale eddy transfer coefficients by K theory. Significant latitudinal and seasonal effects are apparent in calculated ozone perturbations. The near balance between eddy transports of ozone and the mean meridional flux field they induce is shown, as is the dominance of the mean circulation in the hemispheric average of the vertical ozone flux. The concluding discussion on the status of extant models is balanced and useful.

An account of a discussion follows these three papers, and contains a debate involving *inter alios* Thrush, Pyle, Lovelock, Murgatroyd and Scorer.

*Chapter 7, Medical aspects: UV and skin cancer* by R. H. Mole of the Radiobiology Unit at Harwell, provides a description of the two most common but easily cured skin cancers, and of a third rarer but much more malignant tumour, melanoma, and their relationship to exposure in the UV-B radiation range, 280–320 nm wavelength. While many of the two commoner varieties can be ascribed to sunlight, the author points out that for melanoma the available data show latitude-independent temporal and spatial changes in frequency over the past quarter century which reflect altered social conventions of dress and skin exposure. The probable dependence of tumour induction upon the product of skin area exposed, UV flux and specific skin sensitivity means that latitude and environmental amounts of UV are inadequate indices for assessing UV effects. Finally it is pointed out that a small fractional increase in UV levels would result in a small fractional increase in skin cancer, whatever the dose response relationship, and that what matters is the absolute increase in numbers of cases.

*Chapter 8, The fluorocarbon/ozone issue: an industrial view* by R. L. McCarthy and F. A. Bower of du Pont and Co. is a trenchantly stated account from the United States standpoint. It states the main points clearly, emphasizing in particular the importance of the results sought by the atmospheric lifetime experiment and an investigation to determine the total chlorine mixing ratio in the stratosphere. Perhaps too much is expected from statistical modelling of time series of total ozone records from Dobson spectrophotometers; these data are claimed to provide an early warning system for CFM-induced reductions in ozone with a higher sensitivity than is admitted by those who actually make the observations. Finally it is concluded that FC-22 ( $\text{CHClF}_2$ ) is the only alternative compound in sight for FC-11 and 12 ( $\text{CFCl}_3$  and  $\text{CF}_2\text{Cl}_2$ ), but that even this molecule has substantial reservations attached. The authors were unable to resist making the observation that 'In the United States, as you are well

aware, the regulatory decision has been made with regard to aerosols in the absence of compelling science'.

*Chapter 9, Fluorocarbons in the European polyurethane foam industry* by B. M. Grieveson of Shell Research, Amsterdam, is a listing of the usage of fluorocarbons in blowing foams, and is very brief, as befits what in Europe is a relatively low fraction of the total production; nevertheless, no alternative is known, and it is pointed out that the cessation of polyurethane foam manufacture would substantially increase costs in the furniture industry.

*Chapter 10, A Continental European industrial viewpoint* by G. von Schweinichen of Montedison/Milan, is a bluntly stated case for following the European Council of Ministers line: namely, wait for better evidence and understanding but in the meantime permit no increase in production. The evidence is examined and found inadequate as a base for legislation. The US Environmental Protection Agency's concept of essentiality is criticized, and an alternative offered; a rigorous definition is difficult, but the author clearly prefers a stronger role for the market in determining what is essential. It is pointed out that the banning of fluorocarbons would have substantial effects on the supply and demand of several chemicals which are involved as feedstocks and by-products, and would have an economic impact three times as large as the direct losses.

A fourth discussion follows the above papers, succeeded finally by *Chapter 11, Aerosols* by R. A. Gunn-Smith of Metal Box and D. J. Smith of British Aerosol Manufacturers' Association. This is a description, partly historical, of the development of chlorofluoromethane aerosol sprays as 'bug bombs' in the Pacific campaign of the Second World War and their advantages in the post-war market place, as attested by rapid growth to a production of  $6 \times 10^9$  fillings in 1974. Since then production of aerosols has levelled off and declined; alternatives such as hydrocarbons, cheaper but inflammable, are being introduced as bans have been or are to be introduced in the US, Holland, Sweden and Norway.

The book provides a British/European view; it gains from having industry's viewpoint presented, and despite having eleven independent bricks and little mortar does give a fairly coherent picture. What is noticeably missing is an enthusiastic proponent of an EPA-style ban; is it, one wonders, because individuals of sufficient stature holding these views do not exist in the United Kingdom?

A. F. Tuck

## Honours

The following honours were announced in the Queen's Birthday Honours List 1980:

### I.S.O.

Mr C. L. Hawson, who at the time of his retirement was a Principal Scientific Officer in the Special Investigations Branch, Meteorological Office, Bracknell.

### M.B.E.

Mr S. R. Smith, Telecommunications Technical Officer I, Meteorological Office Radar Research Laboratory, Malvern.

### I.S.M.

Mr W. G. Estcourt, Radio Operator, Telecommunications Branch, Meteorological Office, Bracknell.

### Obituary

We regret to record the death on 16 March 1980 of Mr T. Kelly, Senior Scientific Officer, who was stationed at the Main Meteorological Office, Gloucester. Tom Kelly joined the Office as an Assistant Experimental Officer in November 1950 and served at a variety of stations at home and overseas, including tours at Fayid, Negombo (Ceylon), Muharraq and Rheindahlen; he also worked for several years at the London Forecast Office. He was promoted to Experimental Officer in 1954 and to Senior Experimental Officer in 1965. He and his wife Sue, who predeceased him in 1977, will long be remembered for their kindness and hospitality by the many Office staff and their wives who served with them in Germany, the Middle East and the Far East.

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We regret to record the death on 20 March 1980 of Mr T. S. Douglas, Higher Scientific Officer, who was stationed at Prestwick. He joined the Office as an Assistant in August 1941, and served at a number of outstations (including London Airport), and on weather ships. In 1955 he went to Stornoway on promotion to Senior Scientific Assistant. In 1959 he was posted to Prestwick where, with the exception of an overseas tour to Bahrain, he remained for the rest of his life, being promoted to Higher Scientific Officer in 1978. His shrewd and well-informed advice to the forecasting staff at stations controlled by Prestwick was always helpful and will be sorely missed.



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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

The responsibility for facts and opinions expressed in the signed articles and letters published in this magazine rests with their respective authors.

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Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd., 24–28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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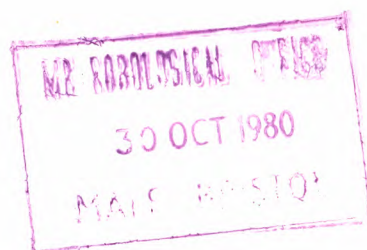
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 9/80

Annual subscription £21.18 including postage  
ISBN 0 11 722065 5  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

October 1980

Met.O. 931 No. 1299 Vol. 109





# THE METEOROLOGICAL MAGAZINE

No. 1299, October 1980, Vol. 109

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551.501.9:551.524.36(428)

## **The northern Pennines revisited: Moor House, 1932–78**

By Gordon Manley

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*Editorial note:* This paper was submitted by Professor Manley in March 1979, and while discussion of it was going on we received the sad news of his untimely death. We are very grateful to Mrs Audrey Manley for making available to us her husband's original MS and other working papers which have enabled us to clarify certain obscurities in the copy as received. The following extracts from his covering letter will, we think, be of general interest:

The circumstances are these. There has been a development of interest of late in the climatology of our higher uplands. The Royal Meteorological Society had a meeting, largely for the younger amateurs, last October, and I was asked to open the affair with the first address. This led me to reflect a bit on the work I set going (nearly 50 years ago!) at Moor House in the northern Pennines—for long the highest place in Britain at which any observations were kept (and long before the days of enormous research grants). It got a notice in the *Meteorological Magazine* in 1932.

The Nature Conservancy took Moor House and its surroundings as a National Nature Reserve in 1952 (largely Prof. Pearsall's work) and have kept good daily observations there. The whole place has a lot of botanical interest in particular. I gave them a digest of my earlier observations (1932–47) but last year when I went up there and stayed for three days or so I reflected that this old series of observations ought to be standardized on to the later set, and so provide nearly 50 years, with the prospect indeed of further extension backward in time for the benefit of the tree-growth people and others.

But making the 'standardization' has been quite a long and tedious job. (Changes of site, instruments, hours of observation, etc. etc.). Still it has been done. One rather surprising feature emerged; the unsuspected and quite large effect of the proximity of the house on the earlier observations. It seems to me that this might well be demonstrated, for the benefit of future workers. Then there is the interesting evidence that the result of such a wide extent of undrained uncultivated wet moorland, at 1800 or more feet, is to *lower* the prevailing air temperature as measured by the usual method in a Stevenson screen. This is most probably a matter of coarse vegetation and soil conductivity (after Brunt 1941) but it raises, or could raise, quite a variety of questions on the possible effect of local environmental changes, that are not merely 'urban building' or 'pollution'.

Hence I have compiled the attached account and commentary, together with a table of monthly means over 48 years which may be of future use for people working on the area.

There are of course lots of other meteorological elements that could be discussed, but some have only been run for a few years, in relation to other researches, and it would seem to be preferable to defer treatment until they have a longer series.

*Postscript.* Northern Pennine snow cover this year is already on the way to breaking records.

The old Scottish record in the 1880s at Dalnaspidal railway station (1420 ft) on the Drumochter gave temperatures but little higher; but with less rainfall and better drainage, opportunities for cultivation were a little better.

In the following text, *m.n.* stands for marginal note by Professor Manley on his original manuscript and editorial additions are indicated by square brackets.

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### Summary

Two series of temperature observations at Moor House, near Crossfell, are compared with each other and with records from other similar sites to provide a homogeneous series of monthly mean temperatures from January 1931 to January 1979. The effects of topography, soil and vegetation are discussed, as is the relationship of mean temperature to snow cover. Comment is also made on the adjustment of monthly mean temperatures at Durham University Observatory to allow for changes in observing practice.

### Introduction

In 1932, whilst engaged in the University of Durham, I was attracted by the northern Pennines, in particular around Crossfell, as the most extensive area of bleak, uncompromising upland that England possesses. For many it provides unusual interest, with the highest inhabited dwellings, roads and settlements, cultivated land, plantations, all close to an evident treeline with climatic stresses and extremes presenting an obvious opportunity for investigation. Late in the January of that year I set up temperature observations at Moor House, a remote shooting lodge and gamekeeper's cottage on the open, treeless and windswept moorland at 1825 ft (556 m) in extreme upper Teesdale. The normal approach (snow permitting) is by car from the village of Garrigill, six miles to the north in the valley of the South Tyne. It was then the second highest inhabited house, with its nearest neighbour four miles distant. A rain-gauge was added in 1935. A note about the station was published in the *Meteorological Magazine* (Manley 1932) and later papers reviewing the observations (Manley 1936, 1943, the latter with illustrations) appear in the *Quarterly Journal of the Royal Meteorological Society*. Further papers relevant to meteorological observation in the area are in the same journal (Manley 1942, 1945) and in *Weather* (Manley 1971). Contributions by Mr W. E. Richardson, who set up an admirable series of upland observations while teaching at Alston (10 miles north of Moor House) from 1951 to 1956, should be added (Richardson 1954, 1955, 1956a, 1956b).

From earlier descriptions it will be seen that temperatures were read from a (weekly) bimetallic Meteorological Office thermograph with the older-style taller drum, set in a Stevenson screen in the walled garth on the south-west of the house and about 25 yards distant (illustration facing p. 260 of *Q J R Meteorol Soc*, 69, 1943). The extremes were in general checked weekly against adjacent thermometers of standard pattern. Daily maxima and minima were read from the edge of the trace and tabulated for the interval 21 h–21 h for comparison with the two nearest stations, flanking the Pennines,

that were generally acceptable: Newton Rigg (559 ft) 18 miles west-north-west and Durham (336 ft) 33 miles east by north. Both are well-exposed stations on rising ground, not subject to frost-hollow minima such as prevailed at Appleby or Houghall.

This record was terminated in March 1941, but in September 1942 I brought down the mercury-in-steel thermograph that I had been running beside the summit of Great Dun Fell at 2735 ft, three miles west of Moor House, and set it up with its bulb in the screen, now erected on top of a 6-ft wall abutting on the house. This was in order to keep the recording mechanism within the window of an adjacent porch about five yards distant. This exposure, on open treeless moorland, ensured plenty of air movement, and evidence led me to think that for the purpose of establishing the prevailing temperature the results would be adequate. It had earlier been found that the westward protection afforded by an adjacent 5-ft stone wall in the original position was desirable, not only on account of the sheep; the ground is soft, and bimetallic-thermograph readings would have been seriously affected by shaking of the screen in the frequent strong winds. The great advantage of thermographs is that an absentee university don can learn much more about the local meteorology.

When it was found that the overall decline of temperature with altitude, either from Durham or from Newton Rigg, agreed closely with that observed elsewhere, e.g. by Buchan on Ben Nevis,\* this seemed an adequate reason to believe that this Moor House temperature record, after reduction, was acceptable. The mercury-in-steel thermograph was finally dismantled in April 1947; soon afterwards the house was vacated, until in 1952 the whole estate was acquired by the Nature Conservancy as an upland reserve. Daily meteorological observations at 09 GMT began in July 1952 and have been continuous since January 1953 (*Monthly Weather Report*). The Stevenson screen was set up on the open moorland about 100 yards east of the house. Subsequently, rate of rainfall, anemograph, soil temperature, evaporation, stream flow, sunshine and radiation measurements have from time to time been added and provide abundant material for discussion that can also be linked with other experimental programs. For example, in contrast to 1932 there are small experimental plantations of hardy conifers, some thriving quite fairly.

From the standpoint of temperature two series of observations thus exist, and the purpose of this paper is to provide an integration. While it was thought that the earlier location near the house would give a satisfactory result in such windswept surroundings, investigation has shown that the earlier temperatures need to be adjusted quite appreciably in order to equate them with the standard of the later series.† The older site cannot now be compared directly with the newer, as a laboratory has been built on it.

### **Reduction of the earlier series to the later standard**

It has therefore been necessary to carry out an elaborate, protracted and troublesome comparison through the nearest available climatological stations. To the east there are overlapping records at Durham (336 ft, 33 miles east by north), Ushaw (594 ft, 30 miles east-north-east) and also at Chopwellwood (445 ft, 28 miles east-north-east) and to the west Newton Rigg (559 ft, near Penrith 18 miles west-north-west), but with serious interruptions between 1948 and 1952. There was a good upland record at Bellingham, 848 ft, 33 miles north by west, from 1908 to 1963; and beyond, at nearly the same altitude there is Eskdalemuir, 54 miles distant but with a roughly similar southerly-slope

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\* *m.n.* B[uchan] adopted  $1^{\circ}\text{F}/270'$ . [This probably refers to *Trans R Soc Edin*, 34, 1890, p. xxii, where the value given is  $1^{\circ}\text{F}/275'$ .]

† *m.n.* The effect of the house in presenting a barrier to radiation loss about 40 ft high and subtending about one-third of the horizon is clearly considerable.

exposure.\* It has already been pointed out that Appleby, 10 miles from Moor House, was not used because, like Houghall near Durham, it lay in a noticeable frost hollow; such exposures can affect the monthly means quite markedly in individual months. It is moreover desirable to make comparisons with stations both to the west and east of the Pennine watershed; weather at Moor House responds mainly to the pattern shown by places to the west, but not exclusively; in some months easterly and north-easterly winds are dominant, and much depends on their strength.

Further tedious difficulties arose because of changes in the terminal hour of observation for the daily maxima and minima. The earlier 21 h–21 h interval was maintained at Durham until 1958. At Newton Rigg 9 h–9 h was used after 1952. At Chopwellwood, Ushaw and Bellingham 9 h–9 h observations prevailed throughout. But when one makes close comparisons of such local differences over a period, evidence of the likelihood of other changes begins to appear. Screens may have been moved a short distance; instruments (or observers) may have been replaced, apparatus may have been added within the screen, external fences may have been erected or removed, ground may have been dug, trees may have grown. It could perhaps become invidious and certainly tedious to go into detail after so long an interval; suffice it to point to my detection of unsuspected thermometer errors at Durham in the later 1920s and their elimination (Manley 1941a). Scrutiny of the *Monthly Weather Report* likewise revealed a serious discrepancy in all minima at Cockle Park over many months in the 1940s.

The overall monthly means, and the mean daily maxima and minima given by the two Moor House series for each month, have been compared throughout with the stations above-named to the second decimal place. It became evident that the differences were consistently less from 1932 to 1947 than they were from 1953 onwards; at all the stations this was demonstrated for every month.†

For effective reduction to the standards of the Nature Conservancy's 9 h observations that now prevail, all the earlier monthly means, which were Fahrenheit, should be lowered by 0.6 °F. The mean daily maxima can be accepted without change,‡ that they were unaffected is an understandable result in such a windswept area.§ But for 1932–47 the mean daily minima that were derived from the screens that were nearer to the house should be lowered by an average of 1.2 °F. The overall mean daily range, 10.8 °F on the older series, thus becomes 11.4 °F, in close agreement with that observed in the present location over 26 years.

This makes an interesting result as its magnitude was unexpected. Clearly the number of 'quiet clear radiation nights' on this upland is more effective than I was disposed to think, remembering the strong and often cutting wind that assailed the daytime visitor on so many occasions. On such quiet nights the large two-storey house to the north-east would form a considerable impediment to outgoing radiation.|| With regard to the location of the earlier screen, this would be more likely to affect the nocturnal minima than the daytime maxima.

Nevertheless I have found the magnitude of the effect surprising, as it is of the same order as that observed in, for example, an open housing area in a modern suburb. It suggests that it is the impediment to outgoing radiation that should be considered, even in what appears to be a windy area. The effect, on minima in particular, of changes in location of the screen in regard to the hangars on a large airfield can be cited. Such moves may appear to be relatively unimportant, but they need to be taken into account when it is a question of argument with regard to slow climatological trends and their possible causes.

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\* *m.n.* There is no satisfactory station to the southward.

† *m.n.* 5.06 21/21 5.68 9/9 to NR [NR = Newton Rigg].

‡ *m.n.* for practical purposes.

§ *m.n.* within the normal limits of error of good thermometers.

|| *m.n.* subtending about one-third of the horizon and standing 35–40 ft high.

It may be asked what precautions were taken to eliminate the lag that must be expected from a bi-metallic thermograph. Extremes were checked against adjacent standard thermometers in the screen, and the ink trace was read to its outer edge. The internal impediment by the thermograph of the screen, of standard Meteorological Office pattern (about  $30 \times 24 \times 18$  inches, by recollection) might have a small but not negligible effect. It may further be asked whether the results during the last four years (1943–47) that were derived from the mercury-in-steel bulb, in the screen on the wall nearer the house, might also be suspected. I was unable to find any residual difference that could be ascribed to this change of instrument and location, sufficient to justify separate treatment. Bearing in mind the small thermometer errors that are likely to occur with Stevenson screen observations in any event, it has seemed to me reasonable to treat all the earlier monthly means alike in assembling the table (Table I).

This table is printed at length as it provides the longest upland series above 1500 ft that we have and that seems likely to continue. The conspicuously marginal climate at Moor House is of decided interest to botanists in particular. The gap May 1947–June 1952 has been filled after application of the mean differences of temperature acquired from the later records between Moor House and the several stations previously mentioned. Some weighting has been given to the resultant according to the prevailing character of the month. When unsettled westerly weather is dominant, for example, Moor House tends to follow Newton Rigg, or even Eskdalemuir, more closely than Durham. Approximations for the year 1931 have been added to round off the first decade. Average daily maxima and minima for each month since 1931 are also available, and frequency of air frost (130 days, 1956–75); average and absolute extremes can be given, but for the earlier years the quotation of extreme minima must be approximate. Extreme minima are interesting; Moor House lies on the gentle slope, within the upper Tees basin, but about 60 ft above the river, whose valley floor, at 1760 ft, is more than half a mile distant to the north-east. Hence the thermometer at the House does not record the lowest temperatures that without doubt occur in the basin. At Moor House the extreme minima are not lower than those observed under exceptional conditions in the lowland valley-bottoms such as Houghall, Haydon Bridge or Appleby.

We do not know what further considerations of the possible vicissitudes of climate on the northern Pennines will arise in future. I have therefore devoted some thought to the prospect of extension of the temperature record, as well as other elements of climate. Several earlier records of value have been kept, notably Allenheads (1360 ft) and Alston (1145 ft). At Allenheads, 10 miles north-east from Moor House, observations probably organized by Thomas Sopwith began about 1836: I have not found them before 1857. They continued till 1876, and were summarized in Glaisher's *Quarterly Returns*. Averages for 1856–95 were computed by Buchan (1898)—see also Bartholomew's 1899 *Meteorological Atlas*. It seems likely that a Glaisher Stand was used. Afternoon maxima on the slope were relatively favourable and night minima were not exceptionally low, the location being well up the valley side. Alston (1880–86), actually Lovelady Shield in the Nent Valley four miles distant and eight miles from Moor House, used a Stevenson screen and reported to the Royal Meteorological Society. Buchan likewise computed and published averages, and added those for Hawes Junction (1883–98) at 1135 ft, a telegraphic station for the Meteorological Office, 25 miles south of Moor House. But we have no northern upland stations to fill the gaps; a 'bridge' of sorts could be made by extrapolation from Newton Rigg and Ushaw College, Durham, West Witton and Bellingham.

I have tried out the relationship between the monthly means at Moor House and the two northern English series that I have compiled for much longer periods: 'Lancashire', representative of the lowland around Preston, since 1753 (Manley 1946) and Durham (University Observatory) since 1847 (Manley 1941a). Decadal differences are reasonably accordant and there is sufficient material to extend 'Durham' back to 1795, a task now in progress. Fluctuations of the mean temperature over the high Pennines can therefore be reasonably assessed at least from 1800 onward (see Table II). The significance of this will

Table I. Monthly means of temperature at Moor House (1825 ft above sea level), 1931-79

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
	<i>degrees Fahrenheit</i>											
1931	30.2	30.7	31.1	37.2	44.0	47.9	51.4	50.0	45.6	40.8	38.0	34.5
1932	36.0	32.1	33.0	34.9	41.7	49.1	52.8	53.5	47.2	39.4	36.2	36.0
1933	30.7	31.4	38.6	40.2	45.6	52.1	55.4	54.5	50.1	42.4	35.0	30.3
1934	34.0	34.9	32.0	37.3	43.2	41.7	56.0	51.1	49.5	41.8	36.4	38.5
1935	33.3	33.1	36.2	37.2	43.7	50.8	54.0	54.0	46.6	40.2	36.9	30.3
1936	31.2	29.5	36.0	35.1	43.7	49.5	50.9	53.5	51.1	41.4	36.7	33.8
1937	34.0	32.0	28.1	39.9	45.9	48.9	54.0	55.0	48.1	43.5	35.7	30.4
1938	34.5	34.0	41.5	38.7	42.3	48.8	51.3	52.2	48.2	42.9	40.7	33.6
1939	30.2	35.1	35.1	39.9	45.8	50.1	52.1	53.7	49.0	39.9	39.8	31.5
1940	25.8	28.8	34.5	39.5	48.1	55.5	50.5	50.9	46.7	41.5	37.9	33.2
1941	23.8	28.6	32.7	34.3	41.1	51.2	53.3	50.8	51.5	42.7	36.8	36.4
1942	27.9	25.2	31.2	39.9	43.2	49.9	50.8	53.2	47.7	43.1	34.9	37.1
1943	32.1	36.1	37.0	41.3	43.9	49.8	52.6	51.1	47.0	43.8	35.9	33.2
1944	35.9	30.6	34.3	42.0	43.4	47.9	52.9	54.2	46.2	40.7	33.8	32.5
1945	25.4	36.7	39.1	41.0	44.7	49.1	55.4	51.8	49.3	47.4	39.9	34.5
1946	30.3	33.8	34.1	41.7	42.4	47.1	52.2	50.0	48.6	41.5	40.8	31.4
1947	29.4	22.0	27.8	37.1	47.0	51.8	54.0	57.0	50.0	45.0	36.5	34.3
1948	32.1	33.0	39.8	40.5	43.1	48.2	53.2	50.8	48.9	43.1	38.6	33.9
1949	34.5	35.0	33.4	42.6	44.9	51.0	54.2	53.5	52.8	44.8	36.5	34.4
1950	32.4	32.0	37.3	36.5	44.0	52.6	52.6	51.5	46.3	41.7	34.7	26.5
1951	30.7	30.2	30.8	35.9	40.5	47.5	53.2	50.5	49.2	43.4	39.6	34.3
1952	28.1	31.1	35.3	41.3	47.4	49.2	53.3	52.3	43.7	40.6	32.2	30.7
1953	33.5	33.3	38.5	36.6	47.5	50.7	51.6	53.0	50.3	42.7	41.1	38.1
1954	31.1	28.1	33.6	38.3	45.9	48.1	49.4	50.3	45.9	45.7	37.5	36.5
1955	30.1	24.7	29.1	41.4	40.7	47.7	55.6	55.3	50.1	41.4	39.1	35.3
1956	31.3	24.7	35.9	36.2	44.8	48.3	52.6	48.0	50.6	43.1	37.1	37.1
1957	35.1	32.1	41.4	39.3	43.5	49.7	53.4	51.7	45.9	44.5	37.7	33.9
1958	30.7	31.5	29.1	37.3	41.9	48.9	52.5	52.9	51.3	44.6	37.5	33.1
1959	27.9	35.1	37.4	40.3	47.1	50.6	53.5	54.0	49.2	47.6	38.3	36.7
1960	31.5	30.9	35.3	40.7	46.7	52.3	50.7	51.1	47.9	43.6	37.0	32.0

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Table 1 continued

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
						<i>degrees Celsius</i>						
1961	-1.2	2.6	4.3	5.3	6.5	9.6	10.3	10.4	10.4	6.7	2.9	-1.5
1962	0.3	0.1	-2.5	3.1	5.9	8.7	9.6	9.7	8.3	6.8	1.3	-0.3
1963	-4.1	-5.3	1.8	3.9	5.6	10.0	10.7	9.5	8.3	6.5	3.4	-0.5
1964	0.9	0.1	-0.3	4.5	8.3	9.1	10.9	10.7	9.3	5.3	3.2	-0.9
1965	-0.9	-0.7	0.1	3.7	6.9	10.3	9.3	10.3	8.5	7.3	-0.1	0.1
1966	-0.6	0.1	2.3	1.5	6.7	10.9	10.5	10.1	9.9	5.8	1.8	0.7
1967	0.5	0.9	1.9	3.5	5.2	9.3	11.3	11.3	9.3	5.9	2.3	0.5
1968	0.3	-3.0	0.5	3.7	4.9	10.3	10.2	10.9	9.3	8.5	2.1	-0.7
1969	1.1	-4.2	-1.9	2.3	6.2	9.5	12.0	12.3	9.4	9.4	0.4	-0.2
1970	-0.5	-2.5	-0.9	1.3	8.3	11.3	10.5	11.9	10.1	6.3	2.9	0.9
1971	1.2	1.2	0.9	3.4	7.0	7.5	12.3	11.1	10.2	7.3	2.3	3.4
1972	-0.1	-0.1	2.1	3.8	6.3	7.3	10.5	10.3	7.2	6.3	1.8	1.8
1973	0.9	-0.1	2.6	2.0	6.8	10.7	11.2	11.3	9.4	5.3	1.7	0.5
1974	1.9	1.5	1.3	3.5	7.0	8.5	10.3	10.5	7.5	3.3	2.7	3.3
1975	2.5	1.3	0.5	3.8	4.7	9.8	12.3	14.2	8.3	6.3	2.4	2.0
1976	1.3	0.7	0.1	3.5	6.7	11.8	12.5	11.8	8.7	6.7	2.3	-1.6
1977	-1.6	-0.5	2.3	2.2	5.5	8.3	11.3	10.6	8.5	7.9	1.5	0.2
1978	-1.3	-2.5	1.9	2.1	7.5	9.1	(10.1)	(10.8)	(9.4)	(8.2)	(4.3)	(-0.5)
Means												(Year)
1941-70	-0.6	-0.9	1.2	3.8	6.7	9.8	11.2	11.0	9.2	6.6	2.6	0.7
												5.1

Sources. 1931-47 from earlier observations set up by G. Manley (*Q J R Meteorol Soc* 1936 to 1943). Now standardized to the Nature Conservancy's station, 120 yards distant, 1953-78. Gaps filled by careful comparisons with other stations. Bracketed values in 1978 and 1979 are estimates based on records from Durham, Lancaster and Eskdalemuir.

Table II. Monthly means of temperature at Durham, January 1941–January 1979.

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Year
<i>degrees Fahrenheit</i>													
1941	31.7	34.9	38.0	42.0	46.7	56.0	61.4	56.7	57.2	48.9	42.1	41.7	46.4
1942	33.3	31.8	38.0	45.4	49.2	56.1	58.1	59.7	54.3	50.1	41.2	41.2	46.5
1943	37.3	42.0	42.7	48.4	50.7	56.1	57.6	57.7	53.8	49.0	41.7	38.7	48.0
1944	41.7	38.0	40.9	48.0	49.8	53.7	58.7	59.7	52.6	47.2	41.3	38.0	47.5
1945	32.7	43.5	46.4	46.3	50.7	55.7	60.7	58.3	55.4	51.3	45.1	39.4	48.8
1946	36.1	41.1	40.7	48.5	48.2	54.8	60.0	56.7	54.8	47.7	45.3	37.4	47.6
1947	35.9	30.2	35.1	45.3	52.6	57.2	60.7	62.4	56.7	50.6	42.3	40.4	47.5 <sup>a</sup>
1948	38.7	39.6	44.9	46.8	49.5	53.9	58.6	56.7	55.1	49.0	43.3	39.6	48.0
1949	40.1	40.8	40.0	48.9	50.9	57.4	61.1	60.1	58.9	51.3	42.8	40.9	49.4
1950	39.2	38.4	44.2	44.0	48.9	59.5	59.5	58.1	53.5	48.6	41.3	33.3	47.4
Mean	36.7	38.0 <sup>b</sup>	41.1	46.4	49.7	56.0	59.6	58.6	55.2	49.4	42.6	39.1	47.7 <sup>c</sup>
1951	36.7	36.8	37.7	42.6	46.4	54.0	59.6	57.3	55.3	48.3	46.0	39.7	46.7
1952	35.0	38.1	41.8	47.2	53.9	56.6	60.6	59.1	50.7	47.0	38.7	36.6	47.1
1953	39.3	40.1	42.0	42.7	53.2	54.5	57.7	59.6	55.9	47.5	46.2	42.7	48.5
1954	36.9	34.2	40.1	44.5	50.3	53.9	56.7	56.1	52.7	51.6	42.6	42.3	46.8
1955	35.2	32.8	36.8	47.6	46.8	53.5	61.6	61.4	55.9	47.3	44.3	39.7	46.9
1956	36.3	32.3	40.3	41.8	57.9	53.3	58.1	54.0	55.3	48.5	42.3	41.1	46.8 <sup>d</sup>
1957	41.1	37.9	45.9	45.5	43.7	56.3	59.5	57.5	52.3	49.6	43.7	39.3	47.7 <sup>e</sup>
1958	35.9	37.9	36.3	44.0	48.7	53.6	58.3	58.7	56.8	49.5	41.9	39.2	46.7
1959	33.3	39.0	43.7	47.0	52.3	57.3	61.1	61.7	56.5	52.8	43.3	40.7	49.1
1960	37.7	37.1	40.8	47.1	52.1	55.7	57.5	57.1	54.3	49.2	42.2	38.1	47.4 <sup>f</sup>
Mean	36.7	36.6	40.5	45.0	50.5	54.9 <sup>g</sup>	59.1	58.3	54.6	49.1	43.1	39.9	47.4
<i>degrees Celsius</i>													
1961	3.9	5.9	8.1	8.3	9.8	12.9	13.9	14.4	14.0	9.7	5.3	1.1	8.9
1962	3.8	4.3	1.9	7.0	9.6	12.5	13.2	13.3	11.8	9.9	4.6	1.3	7.8
1963	-0.5	-1.2	4.7	7.9	9.9	13.5	14.2	13.2	12.0	9.9	6.7	2.9	7.8
1964	3.2	3.7	3.0	8.4	12.1	12.7	14.7	14.3	13.1	8.5	6.3	2.1	8.5
1965	2.4	3.4	3.7	7.3	10.1	13.4	12.5	13.5	11.9	10.0	3.4	2.5	7.8
1966	1.5	3.3	6.2	5.1	10.3	14.3	14.1	13.5	13.3	9.1	5.3	2.7	8.2
1967	3.3	(5.0)	6.5	7.3	8.7	12.9	14.9	14.7	12.5	9.5	(5.0)	3.6	8.7
1968	(3.4)	1.3	5.9	7.2	8.1	13.5	13.2	14.3	13.1	11.6	5.5	2.5	8.3
1969	4.1	(0.2)	2.7	6.5	9.5	13.1	15.9	15.8	13.5	12.2	3.9	2.7	8.3
1970	2.1	2.1	3.6	5.9	11.6	14.9	14.6	15.7	13.7	10.1	6.1	4.3	8.7
Mean	2.7	2.8	4.6	7.1	10.0	13.4	14.1	14.3	12.9	10.1	5.2	2.6	8.3
1971	3.7	4.5	5.0	7.1	10.7	11.2	15.9	14.5	13.7	10.4	5.5	6.5	9.1
1972	3.1	3.3	5.4	8.1	9.8	11.3	14.4	14.5	11.2	9.7	5.5	4.3	8.4
1973	4.0	4.3	6.5	6.0	10.1	14.3	14.9	14.5	12.8	8.5	4.8	3.7	8.7
1974	4.7	4.8	4.7	6.1	10.4	12.3	14.0	14.1	11.1	7.2	5.7	6.6	8.5
1975	5.6	3.8	4.1	7.7	7.9	13.1	15.9	17.5	11.9	8.8	5.3	5.3	8.9
1976	4.7	4.1	4.1	7.1	10.5	15.3	16.4	15.9	12.0	9.7	4.7	1.9	8.9
1977	1.9	3.3	5.9	6.5	9.1	11.5	14.5	14.3	12.3	10.6	5.7	5.1	8.4
1978	1.9	0.9	6.3	6.1 <sup>h</sup>	10.7	12.7	13.8	14.2	13.3	11.4	7.7	2.5	8.5
1979	-0.5	0.9	—	—	8.7	13.5	15.3	13.9	12.5	10	—	—	—
Means 1941–70	2.0	4.5	—	8	9.6	12.6	13.9	14.9	12.5	10	—	—	—
°F	36.8	37.2 <sup>h</sup>	40.7	45.4	50.0	55.7	58.7	58.2	55.0	49.5	42.4	38.5	47.3
°C	2.7	2.9	4.8	7.4	10.0	13.2	14.8	14.6	12.8	9.7	5.8	3.6	8.5

[Some of the means in the above table differ by more than 0.1 °F from those in the original MS. The original values were as follows:

(a) 47.7 (b) 38.6 (c) 47.0 (d) 46.3 (e) 48.1 (f) 47.7 (g) 55.2 (h) 37.4

Parentheses enclosing certain values occur in MS but are unexplained.]

Reference: paper by G. Manley 'The Durham Meteorological Record 1847–1940', table on pp. 370–371 of *Q J R Meteorol Soc*, 74, 1947. This table of adopted mean monthly temperatures was completed for 1941–50 by the late E. F. Baxter. It is here completed for 1951–57 by G. Manley; and with the change in observing routine in 1958 the later values (based on 9 h–9 h daily) are added, bringing the table up to January 1979. A series of careful comparisons has demonstrated that the present-day (9 h–9 h) monthly means are so nearly the equivalent of the older 'adopted means' (21 h–21 h) that they can be used in continuation, as above. For 1847–1940 see reference above.



become evident in regard to estimation of the persistence of snow cover, and of the springtime lag in exceptionally cold seasons that can be critical for all upland production. Not only have we the capacity to make close estimates of monthly means and of rainfall since 1800 if not earlier; the monthly frequency of days with snow falling is available for some part of the Pennine area throughout.

### **Rain, sun, snow and frost**

The area is wet and the wide extent of peat-bog and coarse moorland vegetation shows it. Rainfall has been measured since 1953, and an average of 2010 mm (79.1 inches) for 1941–70 has been computed by the Meteorological Office. A gauge closer to the house was maintained through 1935–40, and another, about  $\frac{3}{4}$  mile distant, between 1876 and 1878. These indicate a similar amount. Sunshine duration is now measured, and in spite of widespread and frequent low cloud the average is more reasonable than one might expect: about 1170 hours (Eskdalemuir 1232, Durham and Newton Rigg, 1330–1350).

For some years a sunshine recorder has also been maintained on Great Dun Fell (2780 ft) on the Pennine watershed three miles to the westward; Crossfell, the highest summit, is four miles north-west of Moor House. Sunshine duration, extrapolating from the few years available, appears to average only about 850 hours, indicating how frequently the summits remain capped although the sun is shining only three miles to the east and 900 ft below.

Snow is an obvious element of interest. Frequency of fall is rather difficult to assess from the limited observations, and can be influenced by the circumstances (Manley 1978). Frequency of snow cover, however, is what most people are concerned to ask about. To the Nature Conservancy's observations I have added the close estimates that I drew up between 1932 and 1940 when I was making frequent visits and gaining experience. Between 1941 and 1952 further estimates derive from the Snow Survey data, together with daily observations by Mrs C. Tudor of Blencarn at the western foot of Crossfell of snow cover on the adjacent slopes, Mr Richardson's valuable reports from Alston, and available reports from the climatological stations at Bellingham and Ushaw. From all this, the overall annual average for 1931–79 can be put at 70 days, ranging in individual 'winter seasons' from 27 to between 110 and 115.\* Precise assessment is not always easy on this wide peaty moorland, gullied here and there and subject to much drifting; a characteristic appearance during thaw is shown facing p. 260 of the *Quarterly Journal of the Royal Meteorological Society*, Vol. 69, 1943.

It becomes interesting to plot the number of days with snow cover for each month against the mean temperature. The variation is quite wide; some cold months may also be dry, some comparatively mild months may have persistent cover after a heavy fall in the preceding month. In general, however, there is an evident linear relationship between days with snow cover and mean temperature November–April. With a likelihood of 33 days at a mean of 2.5 °C the increase is to 115 days at a mean of – 0.5 °C. For the period, 70 days and 1.1 °C are the averages.

If we assume the prevailing lapse rate in our maritime climate to be 0.65 °C/100 m, the equivalent increase with altitude above Moor House in the annual number of days should be about 18 days/100 m. Hence, from an average of 70 at Moor House we might by extrapolation deduce 210 at the summit level of Ben Nevis, which agrees quite fairly with observation. But many variables are involved; for the present, however, it seems reasonable to suggest that the persistence of a general snow cover on our mountain summits is probably more closely related to the mean temperature than to the frequency of occurrence of days with snow falling. There is, however, a broad relationship between the number

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\* *m.n.* 121 in 1978–79.

of days with snow observed to fall at Eskdalemuir, where the standards are those of a first-class Observatory, and the number on which snow lying is observed 1000 ft above at Moor House.

Following an earlier argument (Manley 1949) it would be reasonable to extrapolate from the Moor House observations and suggest that under present climatic conditions small glaciers might be found in the northern Pennines if they attained a little over 1800 m or 6000 ft with a mean annual temperature  $-3^{\circ}\text{C}$  or a little lower.

With regard to days with 'air frost' (screen  $< 0^{\circ}\text{C}$ ), between 1956 and 1975 the annual average is 130. The earlier series of observations, closer to the house, gave 113, sufficiently in agreement allowing for the less open exposure. Air frost has been recorded in every month. Ground frost totals (1957-65) are much higher than those for air frost, an interesting point having regard to the coarse vegetation and lack of drainage. There is some reason to think that the air temperature over this wider stretch of uncultivated and undrained upland is on the whole lower than if the area were reclaimed.

### Local consequences of an exceptional upland environment

The mean temperature of the air at Moor House, as derived from the daily maxima and minima read at 9 h in the Nature Conservancy's screen, is  $5.1^{\circ}\text{C}$  for the period 1941-70. If we extrapolate from the surrounding lowland stations that keep 9 h observations in similar screens, and use the commonly accepted lapse rate for maritime temperate climates such as ours,  $0.65^{\circ}\text{C}/100\text{ m}$  (or  $1^{\circ}\text{F}/280\text{ ft}$ ) we shall find that at the level of Moor House an annual mean of approximately  $5.4^{\circ}\text{C}$  might be expected. This implies that the surface air at screen level at Moor House is for one reason or another cooler than we should expect at that altitude. The argument, however, depends on the extent to which one can accept mean temperatures drawn up on that basis as being strictly comparable.

It is well known that over a decade or more the mean temperature in a well-defined frost hollow, such as Rickmansworth in the past, can be as much as  $1^{\circ}\text{C}$  below that shown by stations at similar altitude nearby.\* Moor House, however, does not occupy a frost hollow; it is on the gentle slope of the broad upland basin of the Tees, about 60 ft above the river which is over half a mile distant. If we were able to tabulate the hourly readings from a continuous record we should know better by how much the mean of the sum of such readings differed from  $\frac{1}{2}(\text{max.} + \text{min.})$ . In general it is lower, but not by the same amount at all types of station, especially if there are buildings in the neighbourhood. We might certainly expect that it would be lower in an upland locality where incoming daytime radiation is more intense, but outgoing radiation towards and after sunset is more rapid.

We must further take into account the nature of the surface soil and vegetation. At Moor House the coarse vegetation of the undrained uncultivated moorland, with many irregular peat bogs and incipient gullies predominates over a great distance. The effect of such coarse vegetation with its matted roots was long ago noticed by Brunt (1945)[?], as the area on which, around an airfield, ground mist would first become visible in the cold surface air soon after sunset on clear calm radiation evenings after showery weather, pointing to the stagnation of a layer adjacent to the ground among the coarser grasses. It has already been noted that the frequency of ground frost at Moor House, compared with air frost, is high.

On balance, it is a justifiable conclusion that this widespread uncultivated undrained vegetation cover provides a reason why the average temperature is lower than we might fairly expect compared with the lowlands. No doubt the lowering of both day and night temperatures when there is a persistent snow cover helps, but this will not on the average operate on more than one-fifth of the days and not all those will provide clear skies and quiet air. Moreover, in extremely cold anticyclonic weather with a lowland snow cover as well, the minima in the lowland valleys are as low as, or lower than, at Moor House;

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\* *m.n. Hawke, Q J R Meteorol Soc, 70, 1944.*

here we need to recall that Moor House itself does not lie in the bottom of the wide upland Tees basin. A more detailed consideration of the daily rise and fall of temperature on such a moorland would appear likely to prove rewarding, together with the maintenance, in particular, of some observations in the flats beside the upper Tees, at about 1760 ft near the main gate of the reserve.

Light might indeed be thrown on the possible changes in the local range of temperature at such a station as Eskdalemuir in the Southern Uplands of Scotland. These uplands have long been known for their very great extent of the pale, coarse mat-grass (*Nardus stricta*) which has been ascribed to regression after centuries of sheep-grazing. Anyone who has paid much attention to incidence of frost and recorded minimum temperatures cannot fail to observe how remarkably low have been many of the past extremes in the Border counties, using the Stevenson screen of the Scottish Meteorological Society, at places such as Kelso, Thirlestane and Stobo Castles, Carnwath, West Linton or Drumlanrig. Above Eskdalemuir in particular there is now a very great acreage of forestry, the consequences of which will be interesting twenty years hence.

The suggestion that if Britain were an uncultivated wilderness it might be colder, to which the Moor House record appears to point, may deserve further consideration. Nothing has been said about the mean daily range of temperature; it is less than at the lowland stations, a result attributable to greater average wind speed, more cloud and less sunshine. The daily range, however, is considerably greater, here in extreme upper Teesdale, than on the summits, as shown by observations on Great Dun Fell nearby, and on other summits such as Fountains Fell (2160 ft) or Lowther Hill (2377 ft). At Moor House the extremes on record to date are 80° and - 3 °F (26.7 °C and - 19.4 °C) but the minimum of - 2 °F in 1947 nearer the house probably implied - 4° or - 5° in the more open exposure now used. There is much that could be added with regard to the several other elements now being measured, but these may well be left to future reviews.

### Addendum

As a by-product of the above investigation, a useful note can be added on the adjustment of the monthly mean temperatures from the long-standing University Observatory at Durham, a site in operation since 1842, free as yet from any urban development. These, as published in the *Monthly Weather Report* (MWR), were derived from 21 h daily extremes until 1957. Because of several changes that had to be accounted for in the earlier record (Manley 1941a) I then derived a series of 'adopted means' for the years 1847-1940, based on a combination of the daily extremes with the 9 h and 21 h fixed-hour readings, a device sometimes used by the Victorians. Fixed-hour means at Durham continued to be published in the MWR for 9 h and 21 h and I have thus been able to continue the 'adopted means', until in 1958 publication ceased. Monthly means based on 9 h daily extremes must now be used, and these should be expected to differ slightly from those derived from 21 h daily extremes. However, close comparison with the other available records indicates that the means as now derived from the 9 h extremes at Durham are so close to the equivalent of any older 'adopted means' that they can be treated as a continuous series; this is a fortunate advantage for climatological discussion since Durham has provided one of our longest British records in one place to have been standardized.

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## Probability forecasts of clear-air turbulence based on numerical model output

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### Summary

During the 1976 Turbulence Survey, pilots' reports of clear-air turbulence (CAT) from about 4500 flights over the North Atlantic and north-west Europe were collected, the main aim being to assess the potential, as predictors of CAT, of various synoptic-scale meteorological indices computed by the operational 10-level model. An earlier assessment of the performance of more conventional, mainly subjective, forecasts of CAT prepared by the Central Forecasting Office (Bracknell) during the Survey, had shown that the probability (per unit distance flown) of encountering moderate or severe CAT within regions forecast to be particularly prone to CAT was only about double the probability of encounter outside these regions. Analysis of the pilots' reports of CAT versus various meteorological indices computed by the 10-level model (rectangle area) indicates that an index combining the predictive abilities of vertical and horizontal wind shear can significantly out-perform conventional CAT forecasts. In addition it is thought that forecasts of CAT must be stated in terms of *probability* (e.g. per 100 km of flight) if they are to convey the maximum possible information to the user.

### 1. Introduction

Reliable operational forecasts of clear-air turbulence (CAT) remain notoriously elusive and little advance has been made in this area of aviation forecasting over the last ten years or so. On forecast significant-weather charts prepared by the Meteorological Office, areas which are predicted to be particularly prone to moderate or severe clear-air turbulence are delineated by the human forecaster following reference to forecast upper-air wind fields, recent aircraft reports of CAT, etc. These CAT forecasts inevitably contain a substantial subjective element, and the relatively large number of synoptic features which, over the years, have been suspected to have some association with CAT, has tended to confuse rather than help the forecaster in his attempts to highlight the likely CAT-prone areas of the chart. In addition, these areas are often so broad (and contain no information on the probability of encounter with CAT) as to diminish a pilot's awareness during flight through these areas.

The intermittent or patchy nature of CAT, both in space and time, militates against the usefulness of this type of yes/no forecast, and in favour of some form of objective probability forecast based on the output of operational numerical models such as the 10-level model (Burridge and Gadd 1977) currently employed by the Meteorological Office. With this aim in mind, a survey of turbulence over and near the British Isles on several days in the spring of 1972 was mounted. Sparks *et al.* (1977) have reported the results of that survey and their main conclusions were:

- (a) that CAT forecasts should be stated in probability terms if they are to convey the maximum possible information to the recipient, and
- (b) that a further survey should be mounted involving a much wider range of synoptic situations in order to establish more firmly and confidently the reliability of any resulting proposed system of objective CAT-forecasting.

Although promising and potentially useful relationships were identified between some 10-level model meteorological indices and turbulence, the relatively small and biased sample of observations gathered during the 1972 survey placed an appreciable limitation on the representativeness of the results.

A further much more extensive survey was therefore mounted in 1976, the main aim being to develop a scheme for objectively deriving forecast probabilities (per unit distance of flight) of encountering

moderate or severe CAT. The main results of the 1976 survey and subsequent analysis are reported in this article.

## 2. The 1976 Turbulence Survey

An extensive Turbulence Survey organized by the Meteorological Office was carried out during the spring of 1976, with the co-operation of the meteorological services and airlines of Austria, Belgium, Denmark, Federal Republic of Germany, France, Republic of Ireland, Italy, Netherlands, Norway, Spain, Sweden, Switzerland and the USA. On ten pre-selected reporting days (9, 12, 15, 18, 21, 24, 27, 30 March and 2, 5 April), spaced at regular 3-day intervals during spring 1976, pilots were issued with specially printed maps covering the North Atlantic (Figure A1 in Appendix) or similar maps covering north-western Europe, and asked to record on them complete turbulence histories of their flights (cruise phase only). Information about the Survey and an example of the type of report required were printed on the back of each (Figure A2 in Appendix).

Pilots' responses were good; a total of 4378 usable maps were received (3.9 million kilometres of flight)—3805 'EUROPEAN' (2.1 million km) and 573 'ATLANTIC' (1.8 million km). Digitizing and quality control (Dutton 1980) of the reports proved a lengthy task. The quality-controlled data have been stored on magnetic tape for analysis; this has included comparison of pilots' (mainly subjective) reports with forecasts of synoptic-scale meteorological indices produced by the operational 10-level numerical model. Some basic statistics of the reports themselves are shown in Tables A1, A2 and A3 of the Appendix to this article; overall, 0.013 per cent of flight distance was reported as severely turbulent (only 8 reports in all), 1.26 per cent as at least moderately turbulent while 9.92 per cent of distance was flown in at least light turbulence. These figures relate specifically to turbulence *in clear air*.

It was also intended to include in this project an assessment of conventional forecasts of CAT, prepared in the Central Forecasting Office (CFO), and issued to aircrew at Heathrow during the 1976 Survey. The results of that assessment have already been reported in detail (Dutton 1979) and are summarized in this article; examples of the distribution of CAT reports in relation to large-scale synoptic features were also presented by Dutton (1979).

## 3. The data for analysis

### 3.1 The synoptic-scale meteorological indices

Output from the operational 10-level model (rectangle area, covering the North Atlantic and most of Europe) was used to compute fields of several meteorological indices believed to have some association with the occurrence of CAT. The indices initially selected for testing as predictors of CAT were wind speed ( $V$ ), horizontal wind shear ( $S_H$ ), vertical wind shear ( $S_V$ ), vertical velocity ( $w$ ), horizontal gradient of vertical velocity ( $\nabla w$ ), vorticity ( $\zeta_a$ ), deformation ( $D$ ); in addition it was thought that some representation of the rate of change (following the flow) of Richardson number ( $Ri$ ) should be included. Following the work of Roach (1970) and Oard (1974) on this particular theme, for the purposes of this analysis the rate of change of ( $Ri$ ) is split into two distinct components, one relating to the rate of change of stability (which we will call  $\phi_1$ ), and another relating to the rate of change of the wind shear term (which we will call  $\phi_2$ ); both  $\phi_1$  and  $\phi_2$  have been included for testing as CAT-predictors. Finally, an index suggested by Dixon (1976), also related to the Lagrangian rate of change of Richardson number, is included for testing (denoted as  $I_D$ ).

All these (11) indices, with definitions in terms of the basic variables computed by the 10-level model, are listed in the Appendix (Table A4).

The constraints of the 10-level model dictate that the smallest volumes for which a forecast index can

be calculated are one grid-length square ( $100 \text{ km} \times 100 \text{ km}$ ) and 100 mb thick. The forecast fields used were those for verification times 06 GMT (18 h forecast based on data time 12 GMT the previous day), 12 GMT (12 h forecast based on data time 00 GMT) and 18 GMT (18 h forecast based on data time 00 GMT) on each of the ten reporting days; the period of validity of each forecast was assumed to be 2 hours either side of the verifying time (that is 04–08, 10–14 and 16–20 GMT respectively).

### 3.2 The CAT forecasts prepared at CFO, Bracknell

For each of the ten reporting days the CAT forecasts included in the 'EUMED' (covering north-west Europe and the western Mediterranean) and 'N ATLANTIC' forecast significant-weather charts for verification times 06, 12 and 18 GMT were digitized on a grid-square ( $100 \text{ km} \times 100 \text{ km}$ ) basis (the grid squares corresponding to those of the fine-mesh area of the 10-level model) by assigning, as appropriate, forecast categories 'MODERATE CAT' or 'MODERATE OCCASIONALLY SEVERE CAT' to each grid square or level lying within regions forecast to contain turbulence. All other grid squares or levels were, by default, assigned 'NIL CAT'. Incidentally, although throughout this article the 'NIL CAT' forecast category is used as the default category, it is never the intention to imply that areas outside those designated as being particularly prone to CAT will be entirely free from CAT. The 'EUMED' chart was used for areas east of  $10^\circ\text{W}$ , while the 'N ATLANTIC' chart was used for areas west of  $10^\circ\text{W}$ .

### 3.3 The pilots' reports

In the digitizing of the pilots' reports, the CAT history of each flight was divided into 'elementary' observations, one for each grid square traversed; each elementary observation included the grid square (10-level model fine-mesh area), height, time, length of track within that grid square, and highest intensity of CAT within that grid square. This format allowed easy comparison of pilots' reported CAT experiences with computed 10-level model indices.

## 4. Results

### 4.1 Relationship between pilots' reports from the same unit region

We will look first at the degree of correlation between pairs of reports from the same unit region ( $xyzt$ ). When more than two pilots reported from the same region, pairs of reports were selected randomly and compared one with another; each report was used only once (a region containing only three reports would, for example, yield just one pair). Table I presents the results of such an analysis for a unit region size of  $100 \text{ km} \times 100 \text{ km} \times z \times 1 \text{ h}$  for two values of  $z$ , 1000 ft and 100 mb, the latter value being the separation between standard levels in the operational 10-level model.

**Table I.** Percentage probability of an aircraft encountering moderate or severe CAT as a function of the CAT experience of another aircraft flying in the same  $100 \text{ km} \times 100 \text{ km} \times z \times 1 \text{ h}$  region (1976 Turbulence Survey).

Vertical dimension of region ( $z$ )	Report of other aircraft in same unit region			
	NIL	LIGHT	MOD or SEV	ANY (background probability)
1000 ft				
	$R_M$			
	1.24 % (0.57)	5.15 % (2.39)	23.13 % (10.71)	2.16 %
100 mb				
	$R_M$			
	1.38 % (0.71)	4.18 % (2.15)	13.81 % (7.11)	1.94 %

This table gives the probability of encountering moderate or severe CAT as a function of the CAT experience of another aircraft flying in the same unit region, and clearly shows that, for this size of unit region, turbulence reports from different pilots are associated sufficiently closely for a report from one pilot to be a useful guide to the turbulence to be expected by another pilot. It is convenient at this point (in reference to Table I) to introduce a simple measure of the usefulness of the association between one pilot's report and another pilot's experience. In the absence of any information (such as a pilot's report or the value of some meteorological index) which is specific to a small region of the atmosphere, the only estimate that can be made of the probability that a pilot will encounter CAT in that region is the frequency with which CAT was reported within regions of the same size in the whole data set. We will call this frequency (expressed as a percentage) the background frequency. For regions with horizontal dimensions of  $100 \text{ km} \times 100 \text{ km}$  the background frequency for moderate or severe CAT varied from 1.6 to 2.2 per cent depending on how the data subset was selected; the background frequency for all CAT (light, moderate or severe) ranged from 11.6 to 13.4 per cent. A pilot's report (or a meteorological index) must be considered useful if it can provide an estimate of the conditional probability of encountering CAT which is significantly different from this background frequency. The ratio of this conditional probability to the background frequency can be used as a measure of the usefulness of the report (or index); the ratio is denoted by  $R_B$  when it is calculated for all CAT (light, moderate or severe), and by  $R_M$  when it is calculated for moderate or severe CAT.

Referring back to Table I, the relevant values of  $R_M$  have been entered in parentheses; it can be seen that, for  $z = 1000 \text{ ft}$ , if the first pilot of a pair reports nil CAT, then the probability that the second aircraft (in the same unit region) will experience moderate or severe CAT is just over half ( $R_M = 0.57 = 1.24 \div 2.16$ ) the background probability of moderate or severe CAT. If, however, the first aircraft reports moderate or severe CAT, then the probability that the second aircraft will experience moderate or severe CAT is more than ten times ( $R_M = 10.71$ ) the background probability. The figures for  $z = 100 \text{ mb}$  (about 5000 ft at normal cruise flight-levels) show that a report from another pilot within the same unit region is still a useful indicator of the probability of moderate or severe CAT within that region. These figures also illustrate well the inherent patchiness or intermittency of CAT, in that, even when one aircraft has reported moderate CAT, the chance of another aircraft (flying at the same flight level, within the same  $100 \text{ km} \times 100 \text{ km}$  grid square, and passing through that grid square within one hour of the first aircraft) experiencing moderate turbulence is no higher than 23 per cent, or about one chance in four.

An investigation of the usefulness of a pilot's report for time separations greater than one hour confirmed the findings of Sparks *et al.* (1977) in their analysis of the 1972 Survey data. In particular, for a  $100 \text{ km} \times 100 \text{ km} \times 100 \text{ mb}$  volume, the ratio  $R_M$  for the case when the first aircraft reported moderate or severe CAT decreased to about 2 for mean separation time between reports of 3 hours (the value for pairs of reports within the same hour was 7.11—see Table I). This result demonstrates how quickly a pilot's report of CAT for a particular location tends to diminish in usefulness as a predictor of CAT at that location.

#### 4.2 Performance of conventional forecasts prepared at CFO, Bracknell

The CFO CAT forecasts ('EUMED' and 'N ATLANTIC' forecast significant-weather charts) for verification times 06, 12 and 18 GMT were compared with pilots' reported experiences within the time periods 0300–0859, 0900–1459 and 1500–2059 GMT respectively, for each of the ten reporting days; a total of 30 'EUMED' and 30 'N ATLANTIC' forecasts were therefore assessed. The results of this comparison are summarized in Table II. Note that, in this contingency table, 'MODERATE CAT' and 'MODERATE OCCASIONALLY SEVERE CAT' forecast categories are combined into a single



**Table II.** *Elementary observations of CAT: CFO forecasts versus reported CAT for all flights (1976 Turbulence Survey).*

Pilot's experience	Category of forecast CAT		
	NIL	MOD or MOD-SEV	ALL
Nil or light	39 630 98.62 %	10 317 97.17 %	49 947 98.32 %
Moderate or severe	555 1.38 % $R_M$ (0.82)	300 2.83 % (1.68)	855 1.68 %
All	40 185	10 617	50 802

category ('MOD or MOD-SEV'), while the pilot-report categories are reduced to 'Nil or Light' and 'Moderate or Severe'.

The numbers of elementary observations falling into each combination of forecast CAT and pilot-experience categories are given, and the percentages here approximate to the percentage probabilities of encountering CAT per traversed grid square. The values of  $R_M$  are given in parentheses. One interesting although not particularly surprising fact that emerges is that 65 per cent (555 out of 855) of encounters with moderate or severe CAT occur within regions not forecast to be particularly prone to CAT (i.e. 'NIL CAT' regions). But the most important result is that the probability of encountering moderate or severe CAT within 'MODERATE CAT' or 'MODERATE OCCASIONALLY SEVERE CAT' forecast regions (2.83 per cent per grid square,  $R_M = 1.68$ ) is about double that within 'NIL CAT' regions (1.38 per cent per grid square,  $R_M = 0.82$ ).

Statistical tests indicate that the apparent degree of skill, albeit rather low, in forecasting areas of CAT (moderate and severe) or NIL CAT is highly significant for both Atlantic and European flights. However, it should be pointed out that strict validity of such tests is conditional on the assumptions of random sampling and normal distribution of the variables. The data presented here satisfy neither condition since:

(a) The definition of the 'elementary' grid-square observations often results in the same (continuous) patch of CAT being counted as two or more 'elementary' observations, one for each grid square traversed within the patch.

(b) Areas of forecast CAT occupy specific synoptic-scale regions; 100 km  $\times$  100 km grid-square categories of forecast CAT therefore obviously exhibit considerable spatial coherence, so that the categories for adjacent grid squares are significantly correlated. This argument also applies, although to a lesser extent, to the actual reports of CAT, since turbulent patches often occur in conglomerates that have synoptic scale.

A more complete account of the performance of CFO CAT forecasts is given by Dutton (1979).

#### 4.3 Performance of individual meteorological indices as predictors of CAT

The performance of each of the eleven synoptic-scale meteorological indices (described in section 3.1) as predictors of CAT was tested by comparing forecast values of the indices for verification times 06, 12 and 18 GMT with aircraft reports within the time periods 04-08, 10-14 and 16-20 GMT respectively. It was decided to divide the 10-day data set into two groups, a 'development' data set (6 days selected randomly) and an 'independent' data set (the remaining 4 days), the intention being to use only the 6-day data set to develop useful relationships between the CAT reports and an empirical combination of the most promising indices (through multiple regression analysis), and then to test the skill of these relationships on the independent data. The 6 days comprising the development data set were 9, 12, 18, 27, 30 March and 5 April, and this data set was used to test the skill of each of the eleven indices

individually. The relationships between reported CAT and a selection of the indices are depicted graphically in Figures 1–7. This type of graphical presentation is the same as that used by Sparks *et al.* (1977) in their report of the 1972 Survey.

The association between horizontal wind shear ( $S_H$ ) and reported CAT (Figure 1) is obviously of some value. We will recall that  $R_M$  is the ratio of the frequency of moderate or severe CAT to the background frequency of moderate or severe CAT, while  $R_B$  is a similar ratio calculated for *any* CAT (light, moderate or severe). For data used in Figures 1–7 the background frequency of moderate or severe CAT is 1.68 per cent; for light, moderate or severe CAT it is 12.45 per cent. Figure 1 reveals that, in general, low values of  $S_H$  are associated with below average frequency of CAT ( $R_M < 1$ ) while relatively high values of  $S_H$  are associated with above average frequency of CAT ( $R_M > 1$ ). For example, for those cases with  $S_H < -2.5 \times 10^{-5} \text{ s}^{-1}$ , constituting about 18 per cent of all cases,  $R_M = 0.19$  and  $R_B = 0.51$ .

Vertical wind shear (Figure 2) also shows an obvious and promising association with CAT. The probability of encounter with moderate or severe CAT increases as  $S_V$  increases. For  $S_V > 7 \times 10^{-3} \text{ s}^{-1}$  (or  $7 \text{ m s}^{-1}$  per km), the probability of encounter with moderate CAT is about three times the background frequency.

Considering that Richardson number ( $Ri$ ), represented in Figure 3 as  $\ln(Ri)$ , is closely associated with vertical wind shear, its degree of association with CAT is a little disappointing. For values of  $\ln(Ri)$  in excess of 1.5 the trend of  $R_M$  (decreasing with increasing  $(Ri)$ ) is as expected, but lower values of  $\ln(Ri)$  ( $< 1.5$ ) show no significant association with the occurrence of CAT.

In Figure 4 the evident lack of any useful relationship between wind speed and CAT may at first sight appear surprising and disappointing but confirms, in association with Figures 1 and 2, that within or near jet streams the local vertical and horizontal gradients of wind assume greater importance (than the wind speed alone) in the well-documented association between jet streams and CAT.

Figure 5 shows that the association between absolute vorticity and CAT is not good. The index may, however, prove useful in identifying regions of *below* average occurrence of CAT.

The association of deformation (Figure 6) and Dixon's index (Figure 7) with CAT is similar to that for vorticity, their main value apparently being in isolating regions of below average CAT probability.

The remaining four indices ( $w$ ,  $|\nabla w|$ ,  $\phi_1$  and  $\phi_2$ ) exhibited no significant association with CAT and the graphs for these indices have been omitted.

#### 4.4 Multiple regression analysis

Multiple linear regression analyses have been used to identify the combination of indices which exhibits the 'best' linear relationship with reported CAT. It was therefore necessary to assign numerical values to reported turbulence intensities, and a simple scale was adopted as follows: NIL = 0, LIGHT = 1, MODERATE = 2, SEVERE = 3. As they stand, many of the indices have relationships with turbulence that are obviously non-linear, and some experimentation with simple modification of some indices (for example, the replacement of  $S_V$  by  $S_V^2$ ) was tried.

The regression program (University of California, Los Angeles 1977) computes multiple linear regression in a stepwise manner, entering the independent variable (index) that best helps to predict the dependent variable (CAT) into the regression equation at each step. The program continues to enter variables until the prediction of the dependent variable does not improve significantly (the level of statistical significance can be selected by the user).

The matrix of correlation coefficients between the unmodified indices is shown in Table III. Correlations with magnitudes greater than 0.3 are in bold type to highlight strongly correlated pairs such as horizontal wind shear and vorticity.

**Table III.** Correlation matrix for the eleven indices (development data set).

Variable	$V$	$ w $	$ \nabla w $	$D$	$\zeta_a$	$S_H$	$S_V$	$\phi_1$	$\phi_2$	$I_D$	$\ln(Ri)$
$V$	1.000										
$ w $	0.217	1.000									
$ \nabla w $	0.155	0.319	1.000								
$D$	-0.026	0.076	0.202	1.000							
$\zeta_a$	-0.085	-0.080	-0.140	0.269	1.000						
$S_H$	-0.134	-0.070	-0.112	0.196	0.784	1.000					
$S_V$	0.386	0.124	0.114	0.065	0.102	0.215	1.000				
$\phi_1$	-0.023	-0.022	0.019	-0.058	-0.013	0.010	0.075	1.000			
$\phi_2$	-0.001	0.009	0.046	-0.031	-0.031	-0.020	-0.057	0.031	1.000		
$I_D$	-0.017	-0.068	-0.139	0.167	0.916	0.679	0.145	-0.028	-0.031	1.000	
$\ln(Ri)$	-0.204	-0.236	-0.270	-0.067	0.172	0.050	-0.654	-0.057	0.035	0.184	1.000

(number of observations = 20 176)

The following empirical index ( $E$ ) has emerged as the 'best' predictor of CAT:

$$E = 1.25S_H + 0.25S_V^2 + 10.5$$

(where units of  $S_H$  and  $S_V$  are as in Figure 1 and Figure 2 respectively). The performances of this index on the development data set and, more important, on the independent data set, are shown in Figures 8 and 9; this graphical presentation is the same as that used in Figures 1-7, except that the results for light, moderate and severe turbulence grouped together ( $R_B$ ) are not included. For comparison the performance of conventional CFO forecasts (categorized as 'NIL' 'MOD') is superimposed (pecked line); this represents the performance of conventional forecasts over the entire 10 days, the figures for which were presented in Table II (section 4.2).

Attempts to improve on the regression equation by the inclusion of topography (as held in the numerical model) have proved disappointing; no significant improvement in the skill of the CAT index was evident.

## 5. Discussion

In the report of the 1972 Turbulence Survey, one of the more tentative conclusions was that forecasts produced by the 10-level model contain information which allows positive predictions of CAT which are about as good as those based only on a recent pilot's report. The results of the 1976 Survey have served to confirm this general conclusion. The 1976 data set was about ten times the size of that achieved in 1972, a much larger area being covered (North Atlantic and north-west Europe) and a much greater variety of synoptic situations being sampled; any conclusions arising from the 1976 Survey must therefore carry a lot more weight than those from the 1972 Survey.

One of the main problems in the interpretation of the results is that the usual statistical tests cannot be reliably applied since they assume random sampling of normally distributed variables. The data used obviously do not satisfy this requirement, owing largely to the considerable spatial coherence exhibited by the variables. However, the 'best' empirical index, derived through regression analysis on the 1976 development data, has been demonstrated to show similarly promising skill when applied to the independent (4-day) data set (see Figure 9); that result is encouraging. The performance of CFO forecasts is relatively poor by comparison. In particular, objective forecasts based on  $E$ , the empirical index, can highlight *small* areas (about 3 per cent of total chart area, on average) of relatively *high* CAT probability (3 to 4 times the background probability), and also have the ability to isolate regions of airspace (about 10-20 per cent of total chart area) with relatively *low* CAT-probability ( $\frac{1}{3}$  to  $\frac{1}{2}$  the

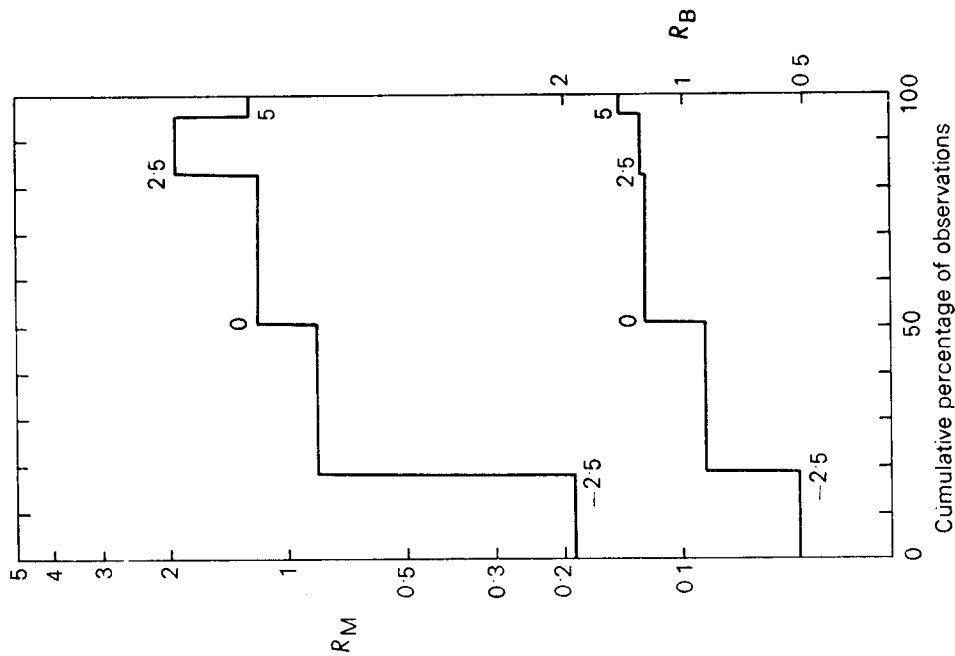


Figure 1. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of horizontal wind shear less than or equal to the values shown on the curve. Units of horizontal wind shear are  $s^{-1} \times 10^{-5}$  (or  $m s^{-1}$  per 100 km).

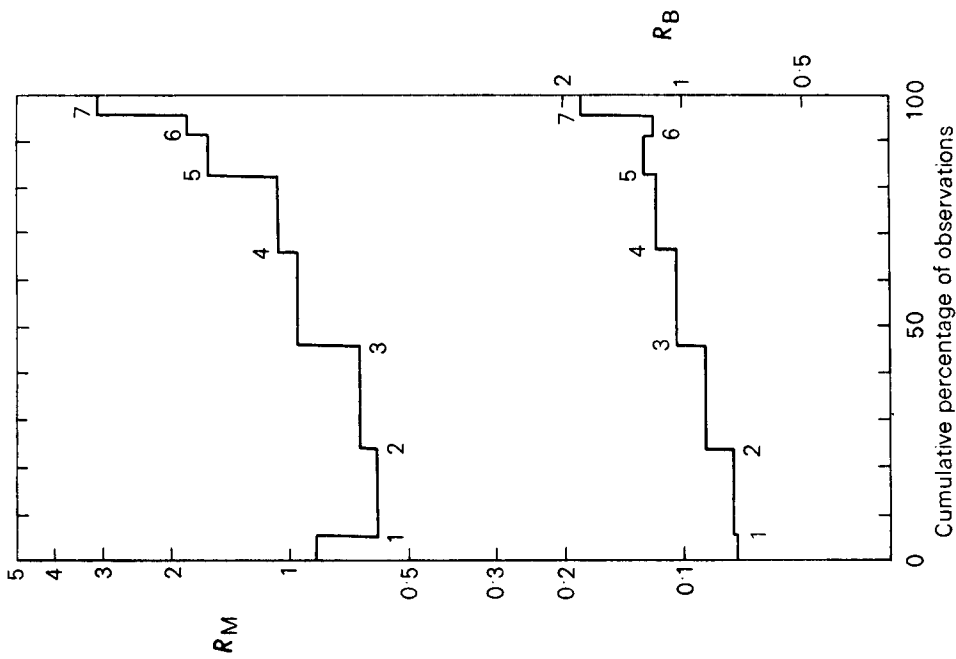


Figure 2. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of vertical wind shear less than or equal to the values shown on the curve. Units of vertical wind shear are  $s^{-1} \times 10^{-3}$  (or  $m s^{-1}$  per km).

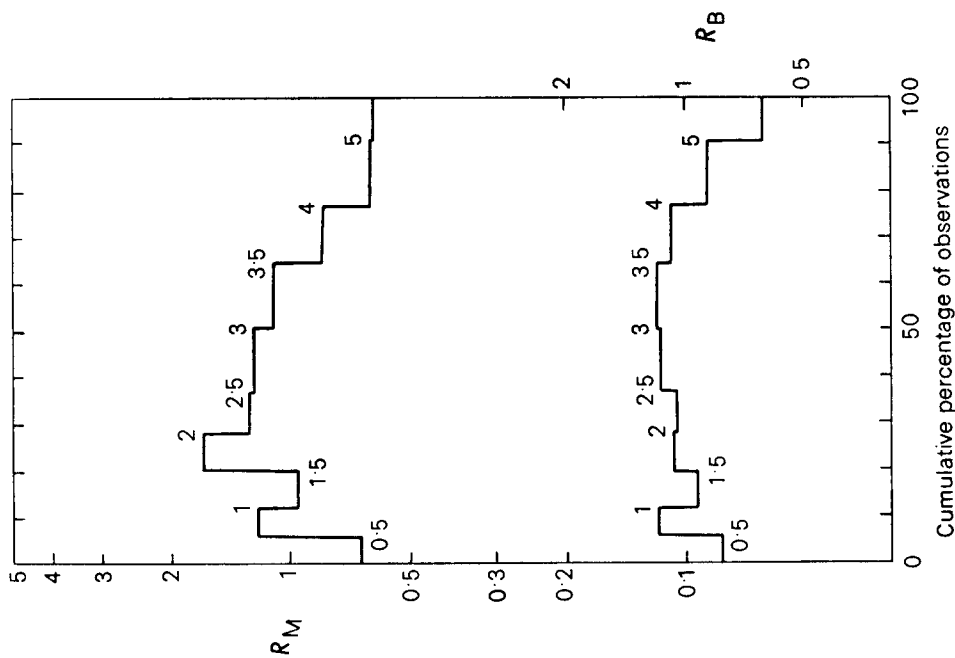


Figure 3. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of  $\ln R$  less than or equal to the values shown on the curve.

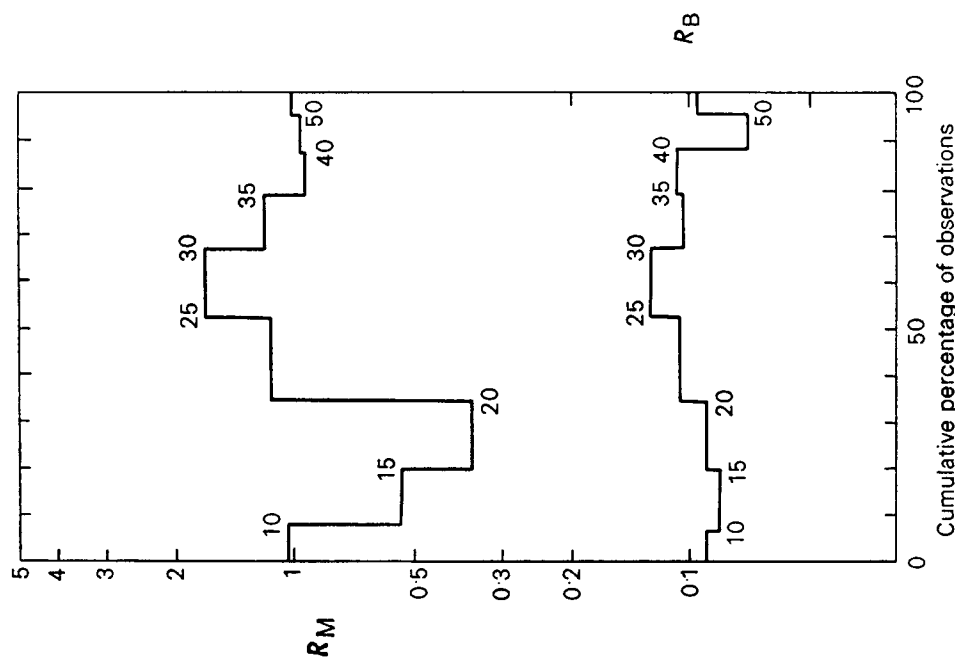


Figure 4. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of wind speed less than or equal to the values shown on the curve. Units of wind speed are  $\text{m s}^{-1}$ .

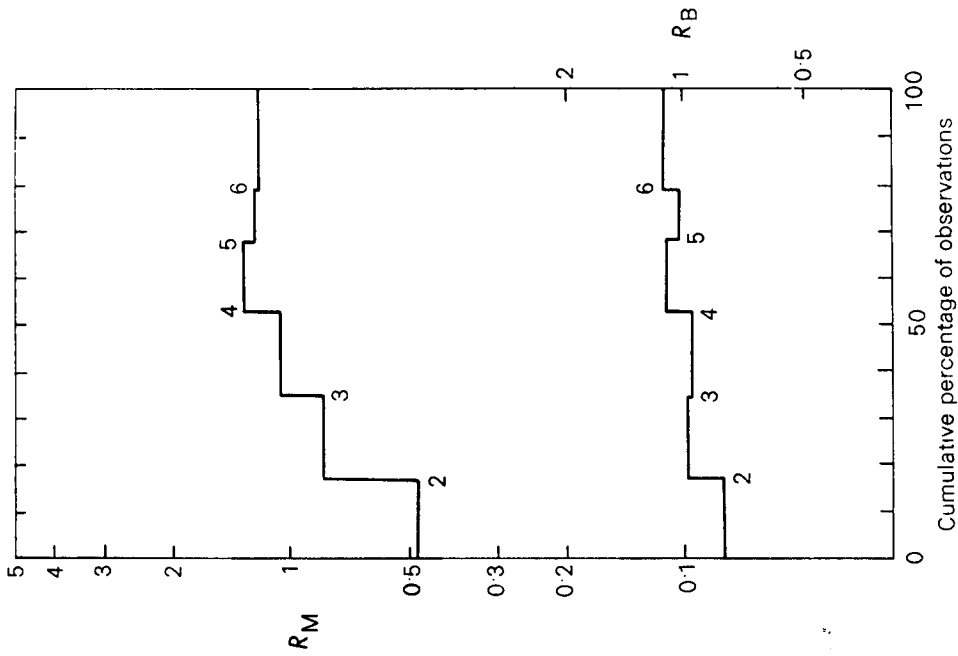


Figure 6. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of deformation less than or equal to the values shown on the curve. Units of deformation are  $s^{-1} \times 10^{-5}$ .

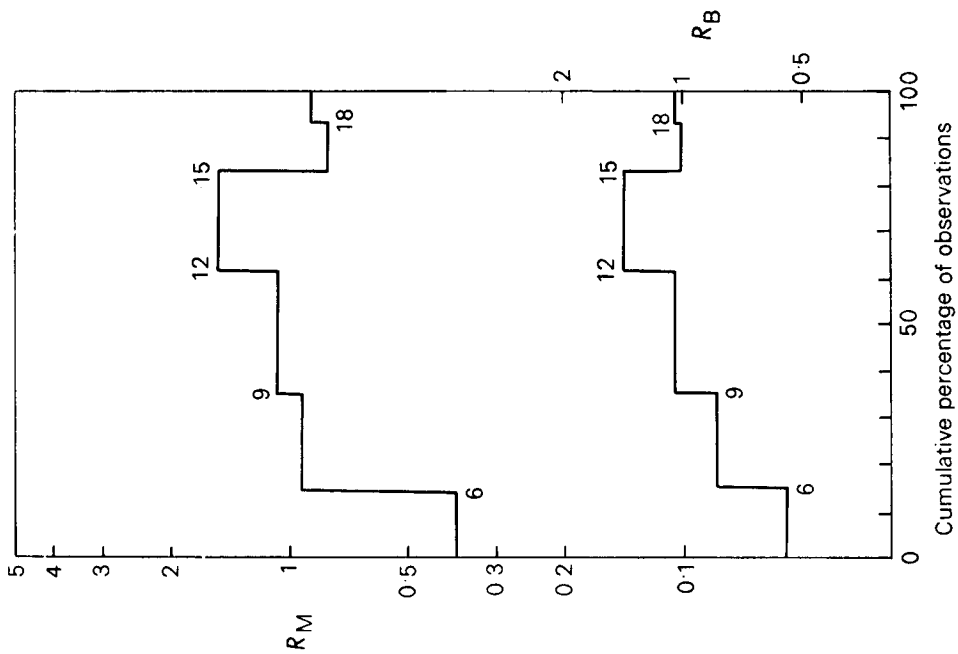


Figure 5. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of vorticity less than or equal to the values shown on the curve. Units of vorticity are  $s^{-1} \times 10^{-5}$ .

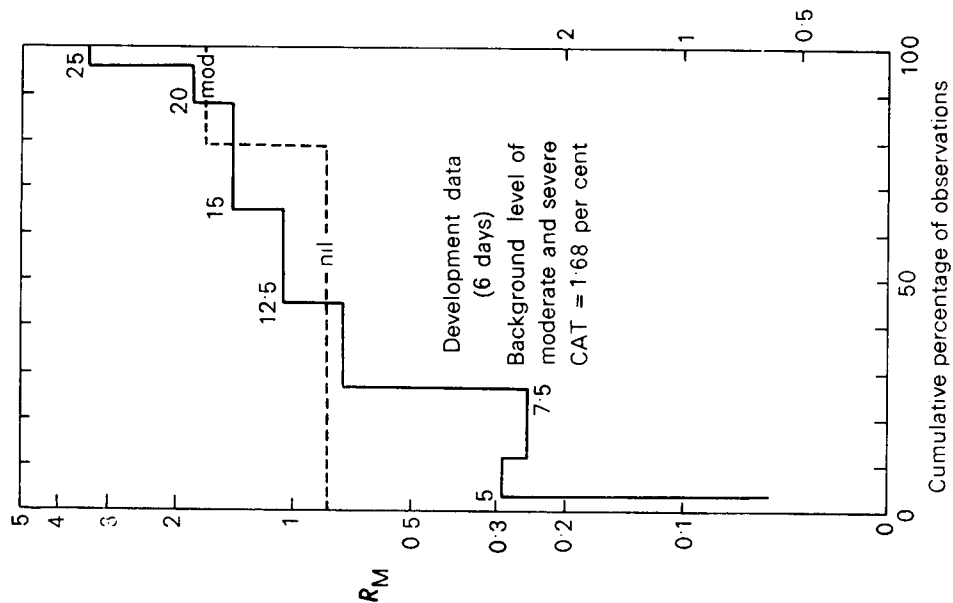


Figure 8. The relationship between the relative frequencies ( $R_M$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of  $E$  (empirical index) less than or equal to the values shown on the curve, within the development (6-day) data set. Pecked line is performance of CFO forecasts (complete 10-day data set).

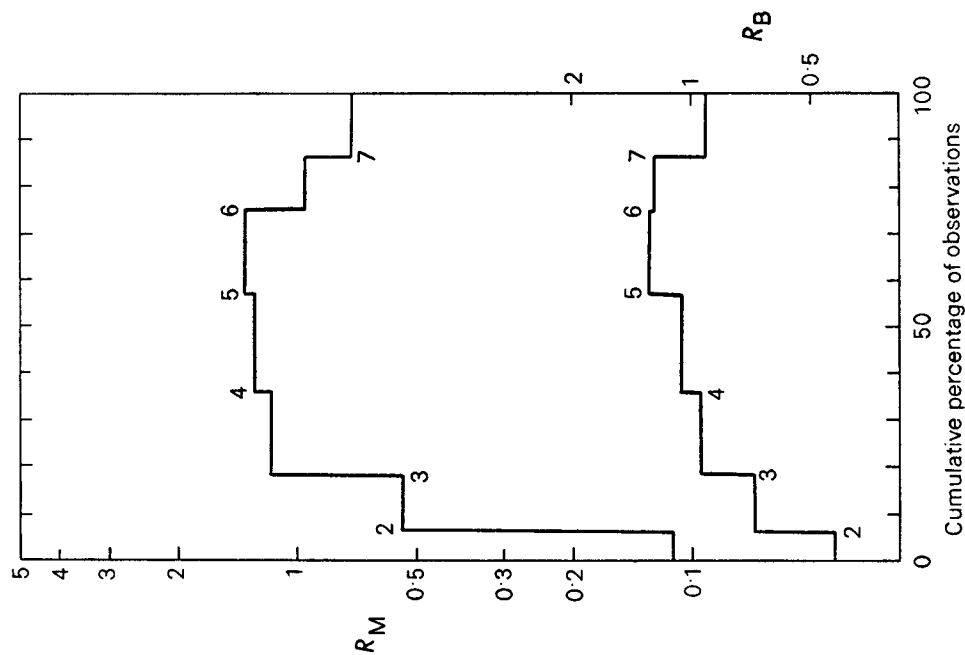


Figure 7. The relationship between the relative frequencies ( $R_M$  and  $R_B$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of Dixon's index less than or equal to the values shown on the curve. Units of Dixon's index are  $s^{-2} \times 10^{-6}$ .

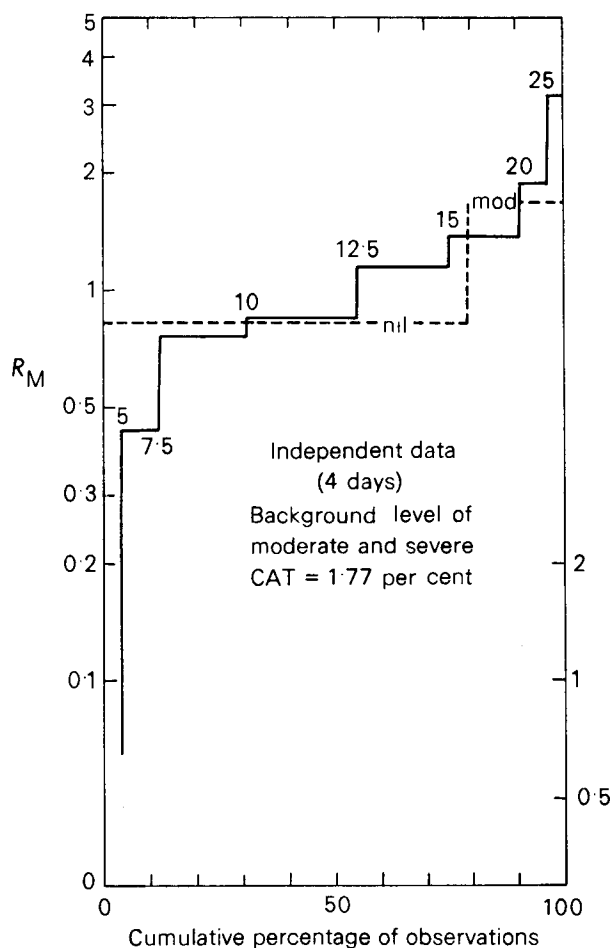


Figure 9. The relationship between the relative frequencies ( $R_M$ ) of pilots' reports of CAT and the cumulative percentage of observations with forecasts of  $E$  (empirical index) less than or equal to the values shown on the curve, within the independent (4-day) data set. Pecked line is performance of CFO forecasts (complete 10-day data set).

background probability). This level of discrimination must represent a significant and useful improvement on the type of yes/no CAT forecast currently used, where as much as about 20 per cent of chart area is forecast to be particularly prone to moderate or severe CAT (with an undefined probability of encounter). The level of skill in these forecasts has been shown to be quite low: aircraft flying in regions forecast to contain moderate CAT actually experienced moderate CAT with a frequency of 1.68 times the background frequency, while those flying outside these regions experienced moderate CAT with a frequency of 0.82 times the background frequency, a difference of a factor of only two between the two forecast categories.

The fact that vertical and horizontal wind shear have emerged as easily the best of the individual meteorological indices tested is not really surprising, since our knowledge of the physical processes leading to clear-air turbulence leads us to expect a good association between CAT and vertical wind



shear; the correlation of CAT with horizontal wind shear is then implied since layers of atmosphere supporting strong vertical shear are often tilted with a typical slope of about 1 in 100, so that a component of wind shear appears in the horizontal. These characteristics of the real atmosphere appear to be well represented in the 10-level model wind fields.

The failure of vertical velocity, its horizontal gradient and the two components of the Lagrangian rate of change of  $(Ri)$  ( $\phi_1$  and  $\phi_2$ ) must be considered conclusive, bearing in mind the size of the 1976 sample.

The remaining indices all possess some small degree of skill, but this lies mainly in their ability to identify regions of relatively *low* CAT probability, an aspect of skill already exhibited by horizontal wind shear (see Figure 1). However, the most important property of any empirical index must be its ability to locate regions of relatively high CAT probability; vertical wind shear is clearly the best individual predictor (see Figure 2) from this point of view. The inclusion of horizontal shear in the regression equation certainly improves the discrimination between regions of relatively high and relatively low CAT probability, but improves only marginally on the skill of vertical shear in locating regions of relatively high CAT probability.

Sparks *et al.* (1977) found that the gradient of vertical velocity ( $\nabla w$ ) was one of the most useful indices in their regression equation, and that wind speed also possessed useful predictive skill. It appears that these associations were a result of the comparatively small and biased sample of synoptic situations covered during the 1972 Survey. Sparks *et al.* (1977), Endlich and Mancuso (1965), Colquhoun (1967), and Bortnikov and Vasil'ev (1974) all found that vertical wind shear performed fairly well as a locator of CAT, but they differed notably in their experiences of the value of other indices such as vorticity, deformation, Richardson number and wind speed. These differences, again, are probably largely attributable to the fact that these surveys dealt with relatively small data sets, each made up of a very limited number of independent synoptic regimes; it is to be expected that some indices perform well in certain synoptic situations and poorly in others. In the 1976 Survey it was the intention that a wide range of synoptic situations should be included; in the opinion of the author, this objective has been achieved.

Trials of objectively derived forecasts of CAT probability, based on computed fields of horizontal and vertical wind shear, are going ahead in the Meteorological Office.

## 6. Concluding remarks

The results of the 1976 Turbulence Survey have shown that objective forecasts of CAT based entirely on numerical model output are potentially useful and perform better than the more conventional type of forecasts currently issued by CFO, particularly in their ability to discriminate between regions of relatively high and relatively low CAT probability. Of the eleven meteorological indices tested as CAT predictors, vertical and horizontal wind shear performed markedly better than any other index.

It is felt that probability forecasts of CAT of the type envisaged (giving grid-point probabilities of CAT per 100 km of flight) will prove more reliable and useful to airlines and pilots than those prepared using current methods. They may, for example, be particularly useful for flight-planning purposes when a choice of routes is available (e.g. across the Atlantic) since overall route probabilities of CAT can be readily calculated, given the grid-point probabilities along the route.

## Acknowledgements

Thanks are due to all the national meteorological services and airlines, most particularly their pilots, who co-operated to make a success of the 1976 Turbulence Survey, and to W. R. Sparks who was

responsible for the planning, organization and implementation of the Survey. The computer programming efforts of C. Passant, S. G. Smith, K. F. Blake, Mrs M. Rowntree and Mrs P. Tonkinson were also appreciated.

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# APPENDIX

**Table A1.** Percentages of distance flown in clear-air turbulence—all flights

Flight level	Light +	Reported CAT Moderate +	Severe +	Total distance flown (km)
<100	9.17	1.09*	0.242*	13 245
100–149	8.29	0.44*	0.077*	38 816
150–199	8.29	0.53	0.061*	111 895
200–249	9.19	0.67	0.018*	236 872
250–299	10.72	1.43	0.006*	686 695
300–349	8.87	0.90	0.012*	1 385 671
350–399	10.92	1.72	0.005*	1 415 676
≥400	5.30	0.24*	0.236*	16 557
All	9.92	1.26	0.013	3 905 427

**Table A2.** Percentages of distance flown in clear-air turbulence—European flights

Flight level	Light +	Reported CAT Moderate +	Severe +	Total distance flown (km)
<100	9.17	1.09*	0.242*	13 245
100–149	8.29	0.44*	0.077*	38 816
150–199	8.51	0.55	0.062*	108 970
200–249	9.31	0.70	0.019*	229 460
250–299	11.25	1.46	0.007*	644 742
300–349	9.73	0.61	—	744 909
350–399	10.82	1.25	—	317 504
≥400	6.01*	0.72*	0.717*	5 441
All	10.21	0.97	0.012	2 103 087

**Table A3.** Percentages of distance flown in clear-air turbulence—Atlantic flights

Flight level	Light +	Reported CAT Moderate +	Severe +	Total distance flown (km)
<100	—	—	—	0
100–149	—	—	—	0
150–199	—	—	—	2 925
200–249	5.48*	—	—	7 412
250–299	2.48	0.91*	—	41 953
300–349	7.88	1.22	0.027*	640 762
350–399	10.94	1.86	0.006*	1 098 172
≥400	4.96*	—	—	11 116
All	9.58	1.59	0.013*	1 802 340

\* Denotes that the percentage is based on fewer than 5 encounters with CAT. Flight levels are expressed in hundreds of feet.

**Table A4.** Meteorological indices computed from 10-level model fields

1.  $V = (u^2 + v^2)^{\frac{1}{2}}$
2.  $w$  computed at grid points by 10-level model
3.  $|\nabla w| = \left\{ \left( \frac{\partial w}{\partial x} \right)^2 + \left( \frac{\partial w}{\partial y} \right)^2 \right\}^{\frac{1}{2}}$
4.  $D = \left\{ \left( \frac{\partial v}{\partial x} + \frac{\partial u}{\partial y} \right)^2 + \left( \frac{\partial u}{\partial x} - \frac{\partial v}{\partial y} \right)^2 \right\}^{\frac{1}{2}}$
5.  $\zeta_a = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} + f$
6.  $S_H = \frac{1}{V^2} \left( uv \frac{\partial u}{\partial x} - u^2 \frac{\partial u}{\partial y} + v^2 \frac{\partial v}{\partial x} - uv \frac{\partial v}{\partial y} \right)$
7.  $S_v = \frac{\partial V}{\partial p} \cdot \frac{\partial p}{\partial z}$  (empirical adjustment applied if level of maximum wind is within 30 mb of a standard level)
8.  $\phi_1 = - \left( \frac{\partial \theta}{\partial p} \right)^{-1} \left( \frac{\partial \theta}{\partial x} \cdot \frac{\partial u}{\partial p} + \frac{\partial \theta}{\partial y} \cdot \frac{\partial v}{\partial p} + \frac{\partial \theta}{\partial p} \cdot \frac{\partial \omega}{\partial p} \right)$
9.  $\phi_2 = - 2Rf^{-1} \left| \frac{\partial V}{\partial p} \right|^{-1} (1000)^{-\kappa} p^{\kappa-1} \cdot F_p$
10.  $I_D = (\nabla \cdot V)^2 + (\zeta_a + f)^2 - D^2$
11.  $Ri = \frac{1}{\rho \theta} \cdot \frac{\partial \theta}{\partial p} \left( \frac{\partial V}{\partial p} \right)^{-2}$

$x, y, z$  orthogonal Cartesian axes of 10-level model

$u, v$  components of horizontal wind ( $V$ ) along  $x, y$  axes

$w$  vertical velocity

$\omega = (\partial p / \partial z)$

$p$  pressure

$\rho$  density

$\theta$  potential temperature

$R$  gas constant

$K = R/c_p$

$c_p$  specific heat capacity of air at constant pressure

$f$  Coriolis parameter

$$F_p = -|\nabla \theta|^{-1} \left\{ \frac{\partial \theta}{\partial x} \left( \frac{\partial \theta}{\partial x} \cdot \frac{\partial u}{\partial x} + \frac{\partial \theta}{\partial y} \cdot \frac{\partial v}{\partial x} + \frac{\partial \theta}{\partial p} \cdot \frac{\partial \omega}{\partial x} \right) + \frac{\partial \theta}{\partial y} \left( \frac{\partial \theta}{\partial x} \cdot \frac{\partial u}{\partial y} + \frac{\partial \theta}{\partial y} \cdot \frac{\partial v}{\partial y} + \frac{\partial \theta}{\partial p} \cdot \frac{\partial \omega}{\partial y} \right) \right\}$$

Vertical derivatives of variables such as  $u, v, w$  and  $\theta$  were determined by fitting cubic splines to values at standard levels (at 100 mb intervals) and computing the vertical gradients *at* the standard levels.

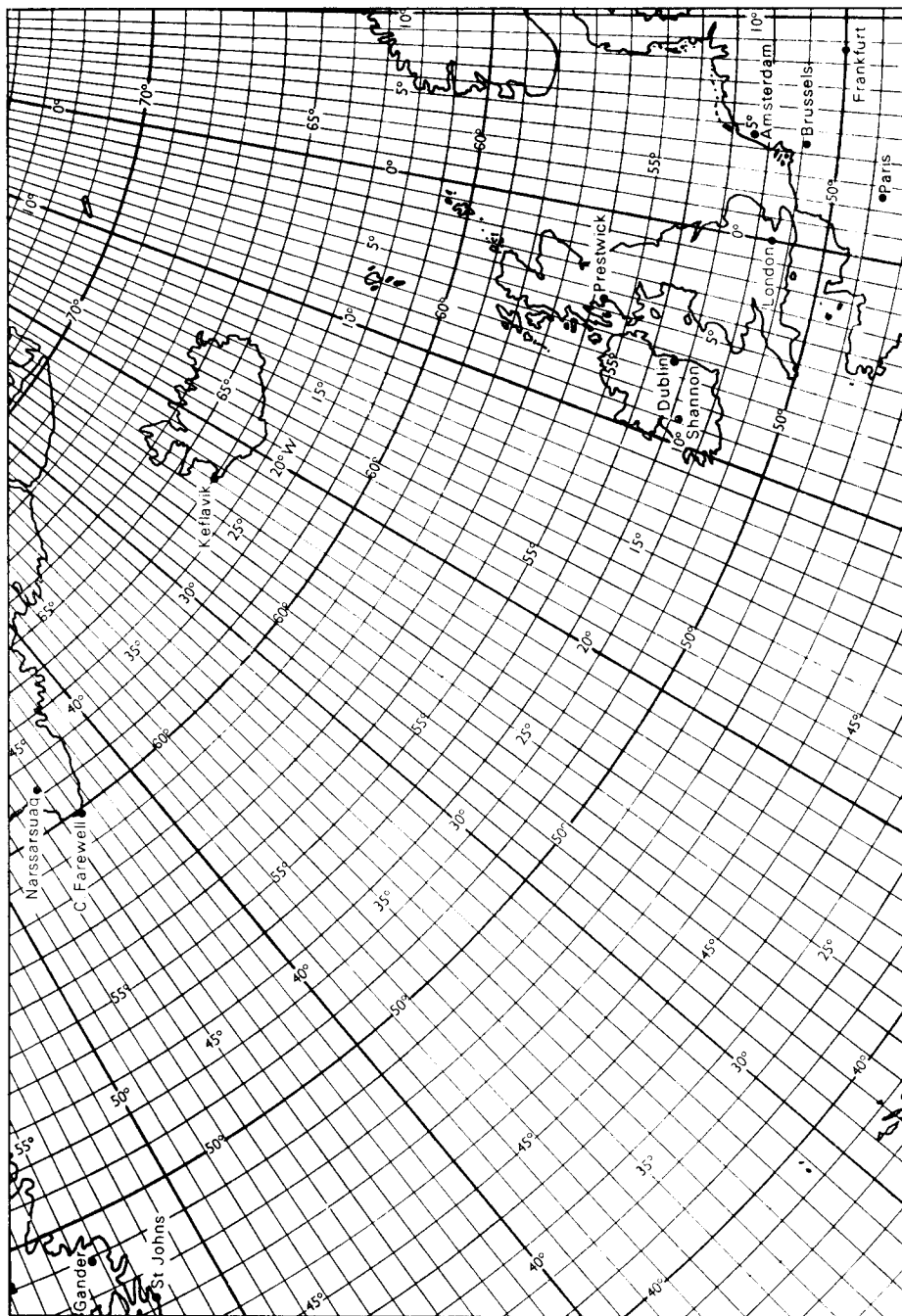


Figure A1. Turbulence Survey (1976) reporting map—ATLANTIC flights.

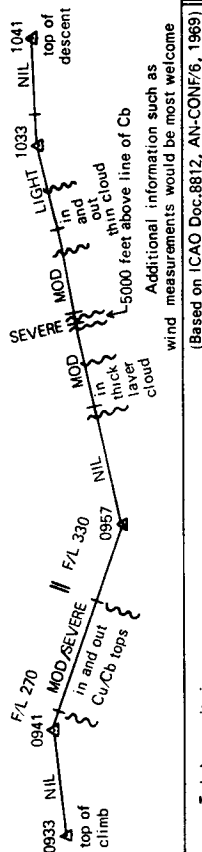
# METEOROLOGICAL OFFICE TURBULENCE SURVEY

These maps are being issued on a few chosen days in order to help assess techniques of forecasting CAT. Captains are asked to complete the flight information section on the right and to describe the turbulence history of the cruise stage of their flight on the map overleaf. All turbulence, whether in cloud or in clear air, should be reported.

## REPORTS OF NO TURBULENCE ARE AS IMPORTANT AS REPORTS OF TURBULENCE

For all points of the cruise stage within the area of the map indicate the track, flight level, and turbulence encountered. Mark the time (GMT) at convenient intervals. When LIGHT, MODERATE, or SEVERE turbulence is encountered show any cloud in the vicinity as in the example below.

### Example



### Turbulence criteria

Description	Effects
Moderate	Moderate changes in aircraft attitude and/or altitude but the aircraft remains in positive control at all times. Variations in air speed are usually small. Loose objects move about. Occupants feel strain against seat belts. Peak changes in accelerometer readings at c.g. of 0.5 g to 1.0 g.
Severe	Abrupt changes in aircraft attitude and/or altitude. Variations in airspeed are usually large. Loose objects tossed about. Occupants are forced violently against seat belts. Peak changes in accelerometer readings at c.g. of more than 1.0 g.
Light /extreme	may be reported when effects are less/greater than these.

Captains are asked to hand maps to a British meteorological office or to their company representative at their destination to be sent to The Director-General, Meteorological Office (Met O 9), London Road, Bracknell, Berks RG12 2SZ, U.K.

### Flight information

Date .....

Aircraft type .....

Company Unit .....

Flight number .....

Departed ..... at ..... GMT

Arrived ..... at ..... GMT

### The reasons for this survey

A CAT survey in 1972 showed that a report of CAT from another pilot within one hour was more reliable than a forecast of CAT, but when the other pilot's report was more than three hours old the forecast was better. The objects of this survey are to improve CAT forecasts and to find ways of combining recent reports from pilots with meteorological forecasts to make CAT warnings more reliable

Figure A2. Turbulence Survey (1976)—Reverse side of reporting map shown in Figure A1.

## Review

*Modification of hail processes*, edited by I. I. Burtsev. 225 mm × 150 mm, pp. viii + 241, *illus.* A. A. Balkema, Publishers, Rotterdam, 1979. Price £6.50, Hfl 27.50, US \$11.00.

The book is an English translation, produced in India, of a collection of Russian articles. The original papers appeared during 1973 and, except for a brief opening survey of French weather modification attempts up to 1971, were motivated by anti-hail operations in the Soviet Union during 1970–71.

The first half of the collection is made up of descriptions of various techniques. The principles and some technicalities of Doppler radar are presented, as are discussions of various properties and uses of cumulonimbus thermodynamics and multidimensional correlation techniques. The information about storm dynamics, which may be inferred from hailstone structure, is set down, although some of the assumptions necessary to put this into practice are far from convincing. In the other substantial section some assessments of the effectiveness of anti-hail operations in the Soviet Union are provided. This is an indigestible mass of information, the value of which has proved impossible to evaluate with any confidence despite attempts to make some sense of the tables and diagrams provided, with maps of the operational areas. Unfortunately the Russian concept of natural and modified hail creation processes, which one might have expected to dominate the book from its title, consists of a single qualitative article, sandwiched between these sections. The evidence for the proposed dynamics and structure of hailstorms, on which the Soviet modification process is based, is referred to but is not presented. The remainder of the book is made up of discussions of the effects of large amounts of nucleating material on the environment and, briefly, of the microwave properties of ice and water spheres.

It is difficult to find any merit in this publication. The basic material must be of intrinsic value only to a very narrow range of English-speaking interests. Furthermore, the articles are obviously not designed as reviews of their subject matter and depend heavily on Russian-language papers which are not generally available. Finally the six to seven years between first publication and translation hardly add to the value of the end product. A study of recent reports of the Swiss experiment (Grossversuch IV) and the National Hail Research Experiment in the USA is likely to provide infinitely greater insight into the realities and problems of hailstorm modification, despite the inconclusive results of these programs to date.

P. Ryder

## Notes and News

### 50 years ago

With deep regret we learn of the disaster to H.M. Airship R 101 with the loss of 48 lives, including that of Mr M. A. Giblett, M.Sc., Superintendent of the Airship Meteorology Division. (*Meteorol Mag*, 65, Oct. 1930, p. 219).

[The *Meteorological Magazine* for November 1930 devoted 16 pages to an account of the disaster and to printing tributes to M. A. Giblett, a scientist of outstanding ability and devotion to duty whose life, in common with those of other victims, was tragically cut short. Dr (later Sir George) Simpson, Director of the Office, in the course of his own tribute to Giblett, wrote: 'The Meteorological Office has lost an assistant of whom great things were expected, an able scientist, and an exceptional organiser'.]

### Mr J. S. Dines

Mr John Somers Dines, the last of a distinguished family of meteorologists, died on 15 May 1980 in his 95th year. His grandfather, Georges Dines (1812–87), invented the dew-point hygrometer; his father, W. H. Dines, F.R.S. (1855–1927), was the inventor of the pressure-tube anemometer and the tilting-siphon rain-recorder and pioneered the exploration of the upper air; his brother, L. H. G. Dines (1883–1965), was for many years Superintendent of Kew Observatory.

J. S. Dines was educated at Emmanuel College, Cambridge and graduated in mathematics in 1906 by which time he had already been assisting his father and brother in their meteorological researches; W. H. Dines was an engineer by profession, and his son gained much invaluable experience of engineering and instrument making by working with him.

J. S. Dines first entered the Office in 1907 as a Student Assistant. In 1910 he was appointed as Meteorologist in charge of experiments for the Advisory Committee for Aeronautics and in 1911 went as Meteorologist to the Branch Office at South Farnborough. In 1915 he joined the Forecast Division and in 1920 was promoted to become Superintendent. In 1929 he was appointed Superintendent of Instruments and of Army Services and he held this position until he retired from the Office in 1939. He published a number of scientific papers during his career and was responsible for writing the second edition of the *Weather Map*, the first edition of which had been written by Sir Napier Shaw; he also investigated the meteorological factors responsible for the very serious flooding of the Thames in London in January 1928, work which led ultimately to the setting up of the Storm Tide Warning Service in 1953.

After his retirement he lived quietly at his home in Hermitage, near Newbury, taking, however, an active interest in local government and church affairs. He retained an interest in meteorology and other scientific pursuits until the end of his life, and letters from him would appear from time to time in *Weather* or the *Quarterly Journal of the Royal Meteorological Society*.





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No. 1299

October 1980

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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'. The responsibility for facts and opinions expressed in the signed articles and letters published in this magazine rests with their respective authors.

Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd., 24–28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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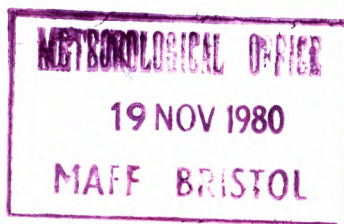
Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd. 698260 K15 10/80

Annual subscription **£21.18 including postage**  
ISBN 0 11 722066 3  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE



HER MAJESTY'S  
STATIONERY  
OFFICE

November 1980

Met.O. 931 No. 1300 Vol. 109

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# THE METEOROLOGICAL MAGAZINE

No. 1300, November 1980, Vol. 109

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## Unusual wave flow over the Midlands

By B. J. Booth

(Meteorological Office, Royal Air Force Lyneham)

### Summary

Glider pilots have for many years used wave flow in the atmosphere, in the form of mountain waves, to climb to considerable heights and fly long distances. However, although mountain waves are probably the most common form of wave flow, evidence has been accumulating in other countries during the last two decades of wave flow originating in different ways. This article describes what is thought to be the first instance recorded by glider pilots of non-orographic wave flow over England. The meteorological conditions existing at the time were remarkably similar to those described during earlier occasions of non-orographic wave flow over Germany.

### Introduction

On 9 May 1979, 29 competitors in the Open Class of the Inter-Services Gliding Championships were tasked with flying a race round a triangular course from Little Rissington, with turning points at Uppingham and Aqualate Mere, near Newport, Shropshire (Figure 1), a distance of approximately 330 kilometres. At the same time a further 17 competitors in the Sports Class were to race around a smaller triangular course with turning points at Ludlow and Bridgnorth.

The forecast promised good gliding conditions with excellent visibility, strong thermals capped by shallow cumulus at 1500–1800 m and a light northerly wind in the convection layer, backing north-westerly at 1500 m. In the event the Sports Class was less fortunate in terms of weather than was the Open Class. Along the first leg thermals were weakened by considerable amounts of 'lenticular' strato-cumulus and, although thermals became much stronger towards Ludlow, an almost complete disappearance of cloud made the task of finding them much more difficult. The Open Class, on the other

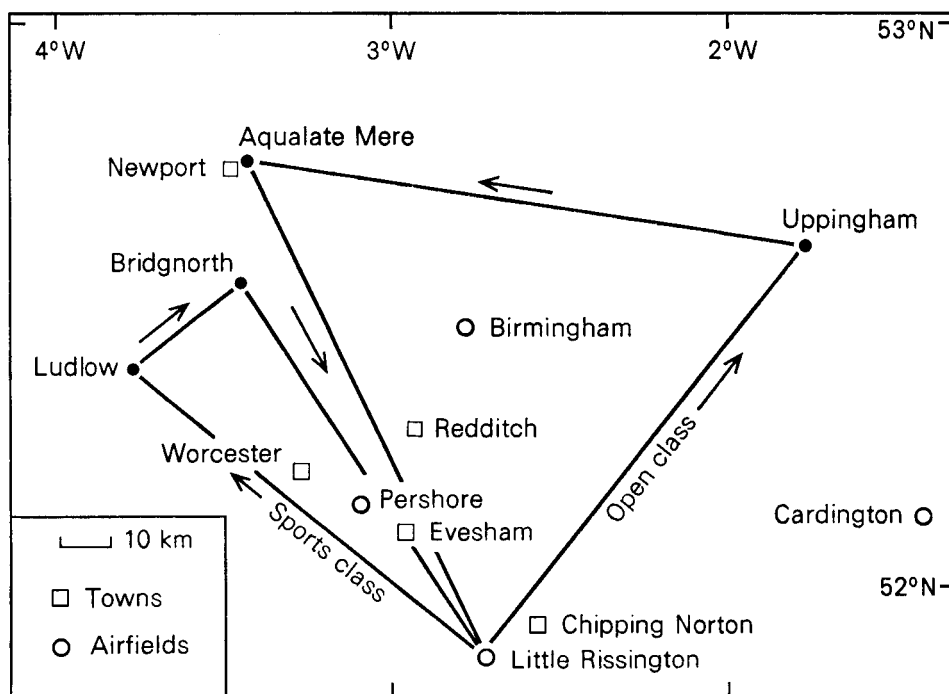


Figure 1. Routes flown by Open and Sports Class competitors on 9 May 1979 between approximately 12 and 18 GMT.

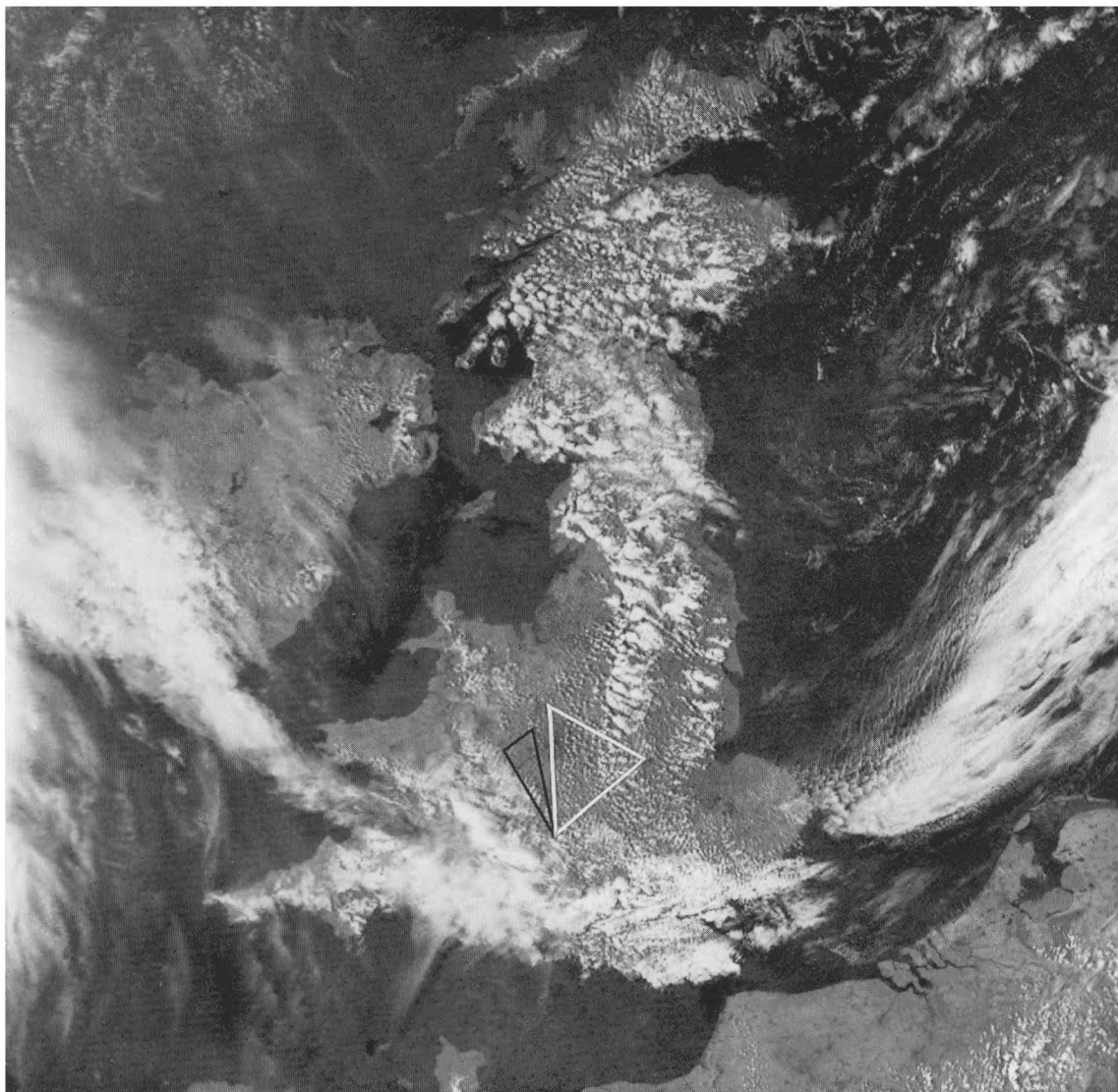
hand, found conditions much as forecast with plenty of thermals beneath cloud streets, although an unexpected reduction in thermal strengths and frequency near Uppingham resulted in a few gliders landing prematurely.

Despite these differences in weather around the two triangles, competitors from both classes were agreed that some form of wave flow was interfering with thermal development during the later stages of the approach to Little Rissington. The Sports Class placed this interference as starting between Worchester and Pershore, while the Open Class placed it as starting between Redditch and Evesham. One Open Class pilot even thought there was wave flow along the second leg, between Uppingham and Aqualate Mere. Clearly, since none of the competitors was able to deviate from the race to explore the conditions, no positive evidence of wave flow could be offered.

However, by fortunate coincidence a non-competitor, Barry Elliot, spent part of the afternoon soaring locally in a K-13, a 2-seat glider, and he confirmed there were indeed waves in the vicinity. Released by a Chipmunk tug at 14 GMT, this pilot climbed in a  $2-3 \text{ m s}^{-1}$ \* thermal from 800 m above sea level to cloud base at 1600 m above sea level. From here he turned west-north-west, into wind, but instead of entering descending air as would normally be expected on leaving a thermal the glider continued to

\* Throughout this article no allowance is made for the rate of sink of a glider since this depends on a variety of unrecorded factors. The minimum rate of sink for contemporary gliders is approximately  $1 \text{ m s}^{-1}$ . Thus the true vertical velocity of the air ascending in thermals is at least  $1 \text{ m s}^{-1}$  greater than the values given.





*Photograph by courtesy of the University of Dundee*

Plate I. TIROS-N satellite picture, 9 May 1979, 1403 GMT. The black triangle indicates the course flown by Sports Class competitors and the white triangle indicates the course flown by Open Class competitors.



rise at  $1-2 \text{ m s}^{-1}$ . This lift increased to a steady  $2 \text{ m s}^{-1}$  and persisted above cloud tops (estimated from the tephigram as 1800–1900 m above sea level) up to at least 2300 m above sea level. Regrettably, by this time, 15 GMT, the pilot had used his allotted flying time and had to land, leaving the upper limit of the lift unexplored.

The clouds as seen from the ground were cumuliform, but the pilot observed that the leading edges of all the clouds in the vicinity were lenticular. This effect was subsequently visible from the ground, and by 16 GMT what few clouds remained had become distinctly almond-shaped.

Following this flight there was another report of a glider climbing to about 2000 m above sea level over Chipping Norton, some 12 km to the north-east, but no further details were forthcoming.

Figure 2 is a diagrammatic representation of the chart from the barograph carried by one of the competitors in the Open Class, Air Commodore Cooke, who was flying a Libelle glider. (In competitions all competitors have to carry barographs.) From this it is possible to deduce that even as late as 17 GMT scattered thermals were giving lift of  $5 \text{ m s}^{-1}$ . Air Commodore Cooke noticed no evidence of wave flow.

Using a TIROS-N picture timed at 1403 GMT, the cloud-street spacing over the Midlands can be measured as 5–6 km. The stratocumulus and subsequent lack of cloud over the Sports Class task area are also clearly shown (Plate I).

A weak, southward moving cold front passed through Little Rissington at about 09 GMT and subsequently became almost stationary as pressure rose to form a shallow low over south-east England (Figure 3). The front is clearly evident on the 1403 GMT TIROS-N picture as a narrow band of stratocumulus over the Bristol Channel and a wider one over the southern North Sea. Convection has caused breaks to develop in the stratocumulus sheet inland but there is a distinct change from the cloudy moist air over southern counties to the relatively dry air over the Midlands. Detail of the low cloud over Somerset, Dorset and Wiltshire is obscured by cirrus, but with no cold advection shown on the 1440 GMT Larkhill pilot-balloon ascent (Table I) the front must still be to the north of Salisbury Plain.

**Table I.** *Larkhill pilot-balloon ascent, 9 May 1979, 1440 GMT*

Heights in metres above ground level, speeds in metres per second					
Height	Direction	Speed	Height	Direction	Speed
0	275	2.5	1050	300	5.5
150	280	3.0	1200	295	5.5
300	270	2.5	1350	295	6.0
450	285	3.5	1500	295	7.5
600	295	3.5	1650	290	7.5
750	295	4.5	1800	280	7.5
900	295	5.0			

The western extent of the stratocumulus, described by some competitors as 'lenticular', can be placed over Radnor Forest (Figure 4) where the highest ground rises to about 660 m above sea level. This particular area of stratocumulus extends some 85 km to the east-south-east before petering out over the Severn valley, but it should be noted that a similar pattern has developed in the stratocumulus further east where convection has broken up the cloud sheet.

Cloud-free areas near coasts are attributed to the inland penetration of sea air, while just inland of the south coast of Wales a sea-breeze front marks the convergence zone between the onshore south-westerlies and the opposing northerlies (Figure 3(b)).

Upper-air data for 11 GMT indicate winds of  $280^{\circ}$ – $290^{\circ}$ ,  $6-8 \text{ m s}^{-1}$  at 1500 m above sea level

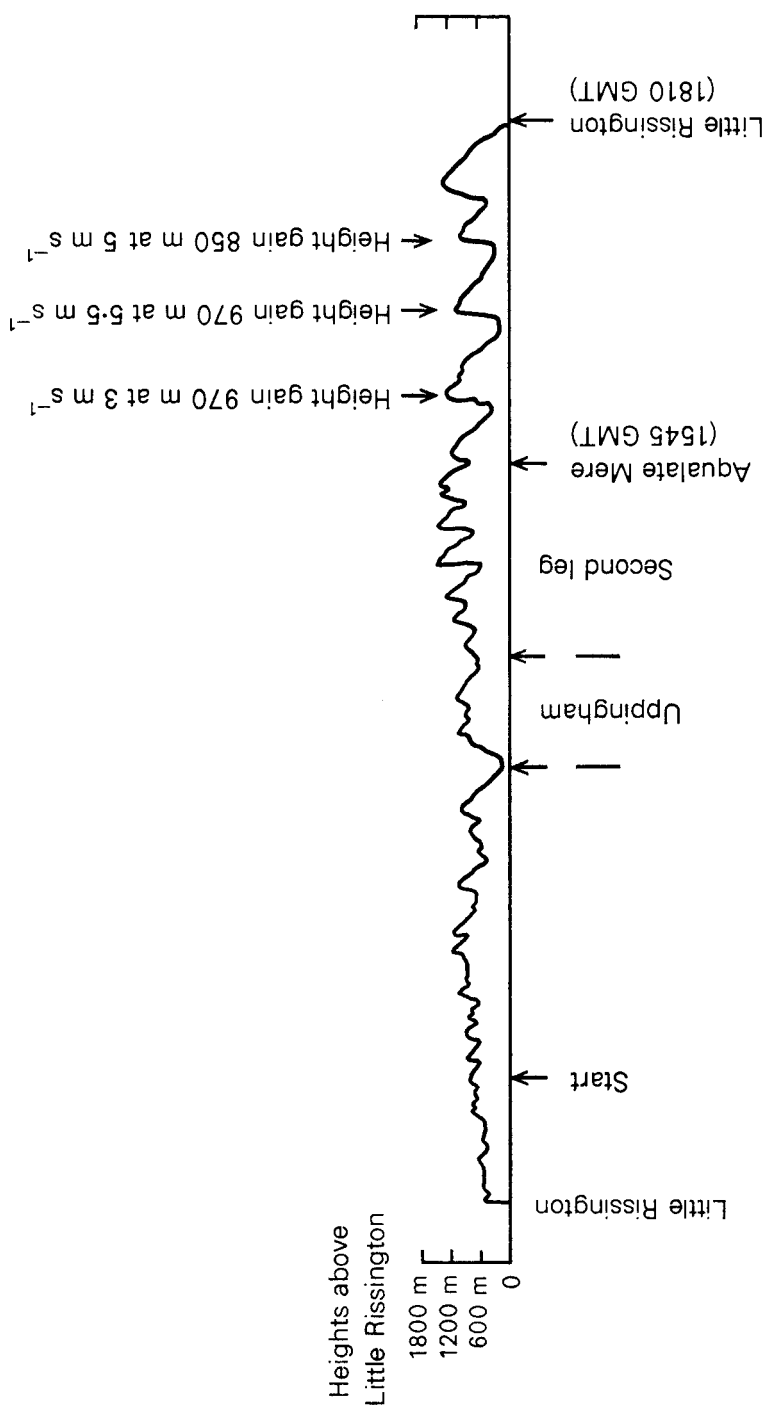
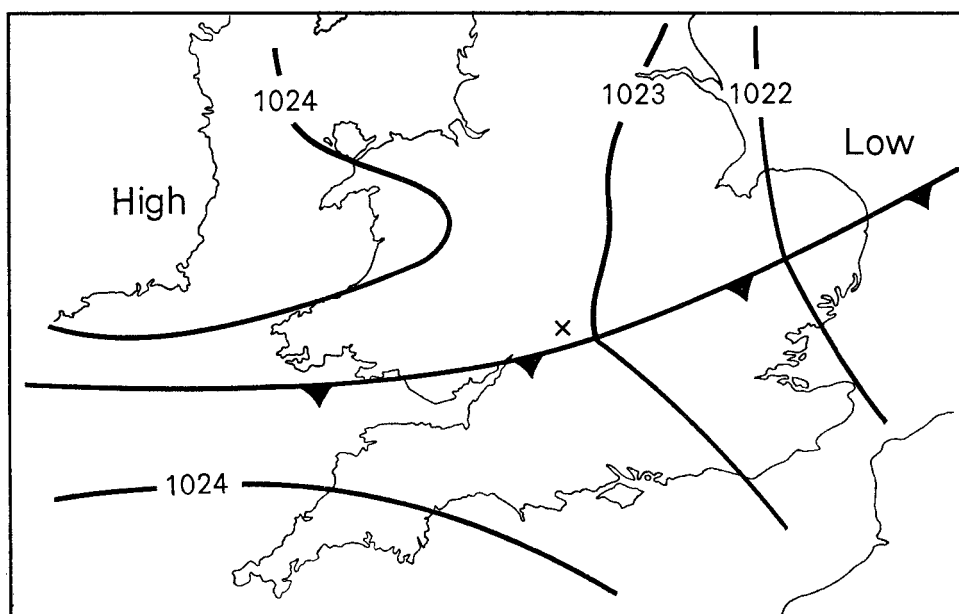
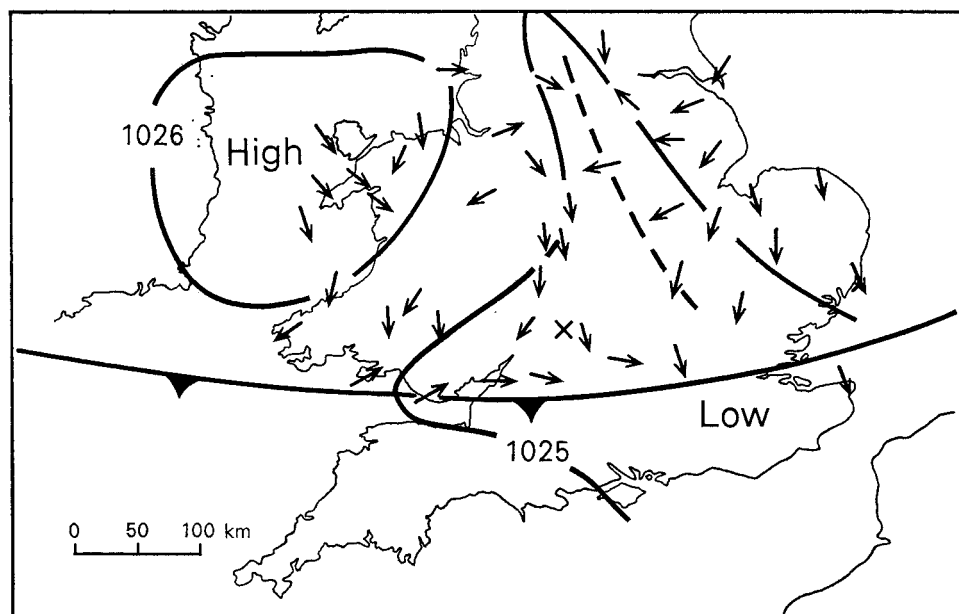


Figure 2. Diagrammatic representation of the chart from the barograph carried by Air Commodore Cooke.



(a)



(b)

Figure 3. Synoptic situation at (a) 09 GMT and (b) 15 GMT on 9 May 1979. Arrows indicate surface wind directions reported at 15 GMT. Cross indicates Little Rissington.

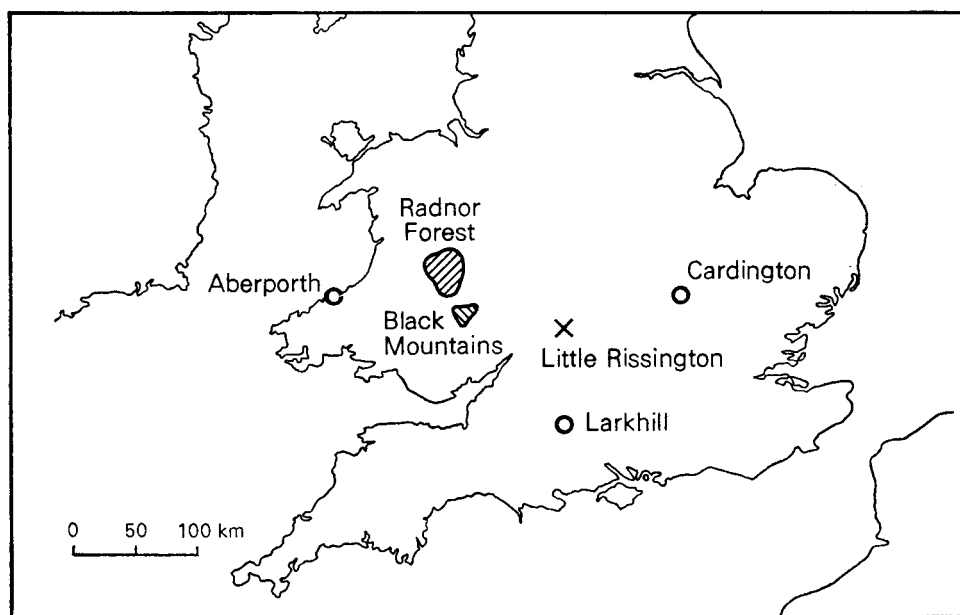


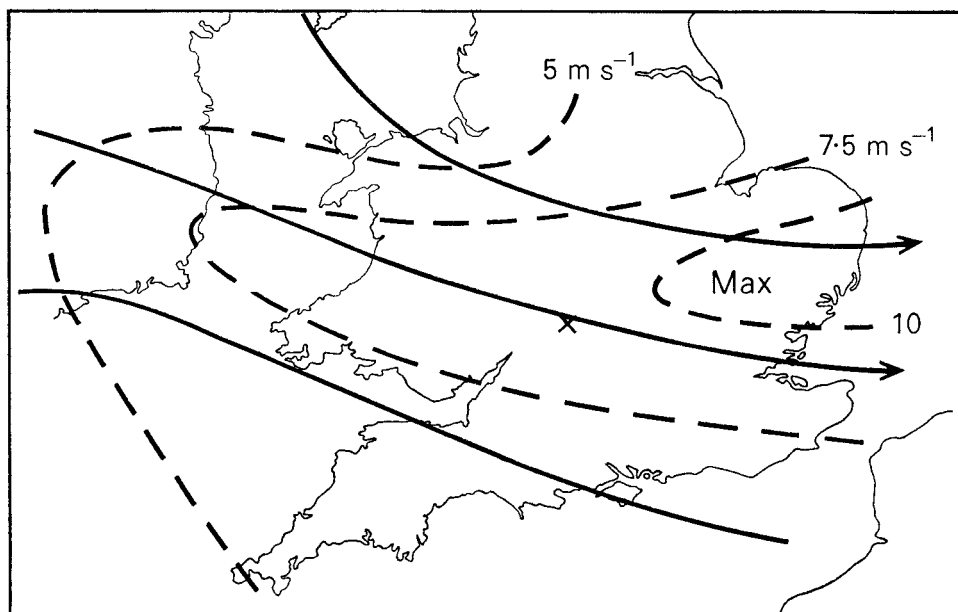
Figure 4. Showing places referred to in the text.

(Figure 5). Although streamline analysis suggests a slight backing and moderation at 1500 m by 17 GMT little change seems to have occurred above, which is consistent with pilots commenting on a 'pretty fresh' wind at gliding altitudes west of Birmingham. Figure 6 shows the horizontal temperature gradient at 1500 m at 11 GMT and, in view of the slack pressure gradients, it seems reasonable to assume that this pattern persisted during the afternoon.

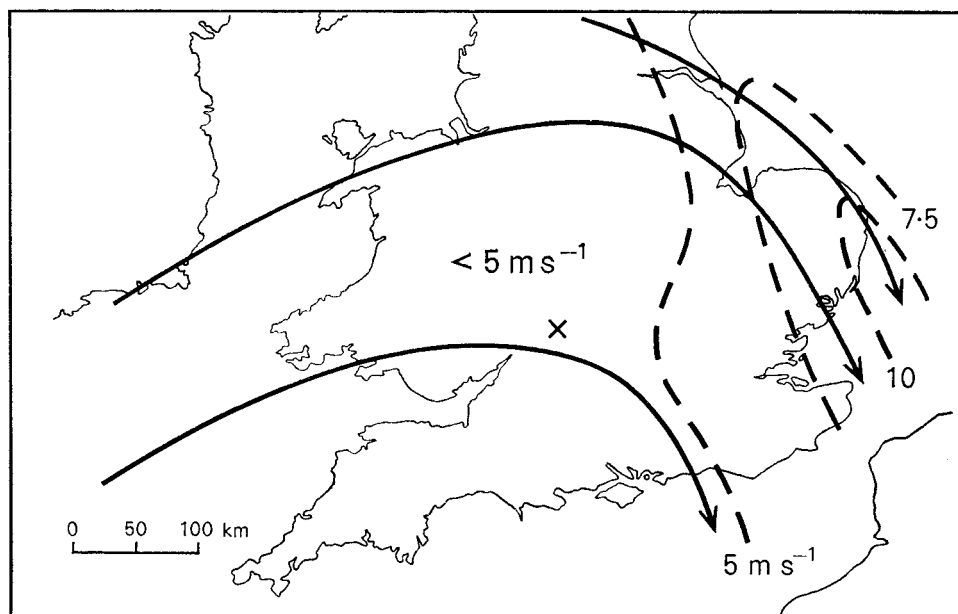
The absence of comprehensive upper-air data over the Midlands makes any detailed assessment of the vertical wind profile in the lower part of the convection layer subjective, but the persistent light north-north-westerly surface winds indicate an organized northerly flow aloft. Reference to wind directions estimated from the 'weathercocking' of the captive balloon during the 1455 GMT Cardington ascent (Table II) shows this northerly flow to extend to about 750–800 m above ground level before the westerly winds at higher levels begin to dominate. While the proximity of a weak convergence zone (Figure 3(b)) means the Cardington winds are not truly representative of the Midlands, the level of the change to the westerly is probably significant. Quite clearly, a marked wind shear existed between the extremities of the convection layer and Figure 7 is thought to be a close approximation to the actual vertical wind profile of the area under discussion.

**Table II.** *Cardington wind, 9 May 1979, 1455 GMT*

Heights in metres above ground level, speeds in metres per second					
Height	Direction	Speed	Height	Direction	Speed
0	N	2.5	450	NNE	4.0
75	NNE	4.0	600	N	2.5
150	N	3.0	750	N	2.5
225	NE	3.5	900	W	1.0
300	NNE	3.5			



(a)



(b)

Figure 5. Streamline and isotach analyses for 1500 m above sea level at (a) 11 GMT and (b) 17 GMT on 9 May 1979. Cross indicates Little Rissington.

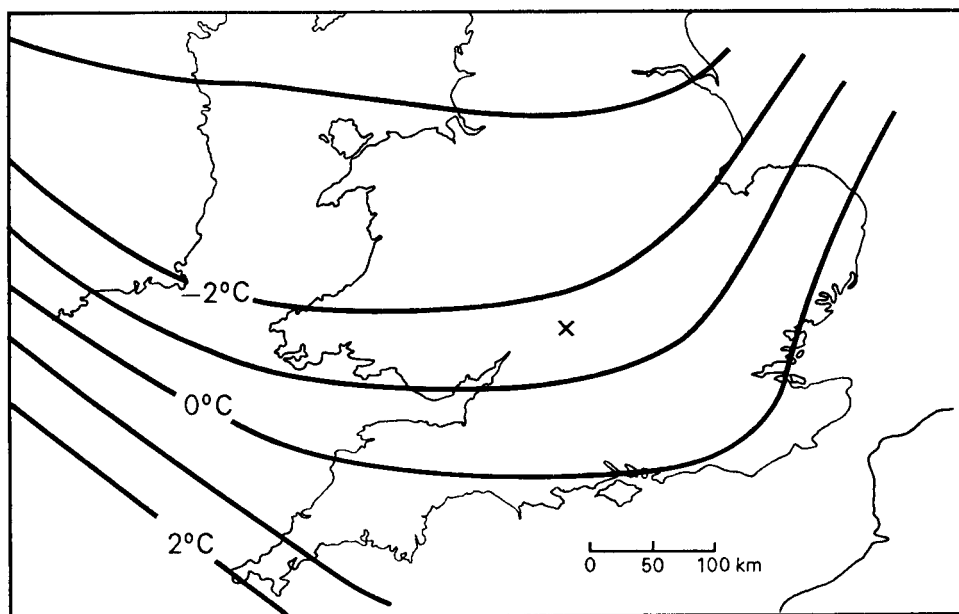


Figure 6. Isotherm analysis for 1500 m above sea level at 11 GMT on 9 May 1979. Cross indicates Little Rissington

The vertical temperature trace of the Cardington ascent shows no evidence of modification by the convergence zone and is considered representative of the change due to heating inland of the air in the lower part of the Aberporth ascent (Figure 8).

## Discussion

Wave-like flow in the atmosphere originates in a variety of ways, but in each there is a common factor in that a hydrostatically stable layer is present in the lower atmosphere.

The lee, or mountain, wave is perhaps the most widely discussed type of wave flow (for example Nicholls (1973) or Ruddock (1970)) and, so far as glider pilots are concerned, is the most frequently encountered. Lee waves, which are caused by wind blowing across a ridge, can extend through great depths of the atmosphere and contain powerful vertical currents which are sometimes hazardous to powered aircraft (Royal Meteorological Society 1965).

At the other end of the scale is the inversion wave which occasionally develops in a shallow layer at a temperature inversion across which there is a vertical wind shear (Reichman 1978). The vertical currents in this form of wave flow are rarely strong enough to enable a glider to maintain altitude, but occasionally a lift of  $1 \text{ m s}^{-1}$  is encountered.

Smooth lift is sometimes found on the windward side of individual cumulus clouds. Kuettner (1972) calls this cumulus wave and notes that the lift does not often extend much above the cloud top.

Finally there is the cloud-street wave, which is similar to the cumulus wave but more powerful and extensive. This form of wave flow is a consequence of wind at and above the inversion level blowing

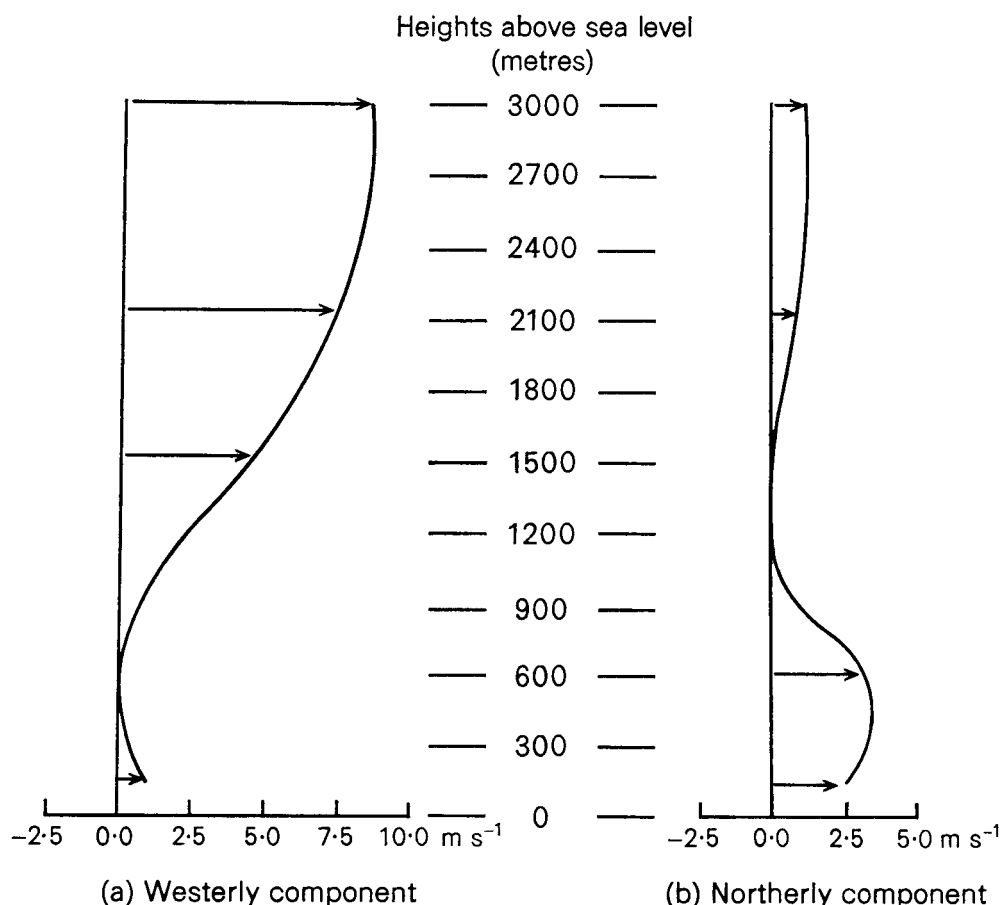


Figure 7. Vertical wind profile over the Midlands during the afternoon of 9 May 1979.

across cloud streets—and hence the low-level wind. Jaeckisch (1972) summarizes the synoptic conditions favourable for cloud-street wave flow as weak flow in the convective layer and a horizontal upper-air (850 mb) temperature gradient with the isotherms directed approximately at right angles to the surface isobars, the upper flow being across the cloud streets. Besides these conditions, Reichman (1978) noted that a further contributive factor in cloud-street wave formation is for the wavelength of the air above the cloud to be similar to the cloud-street spacing. (It could possibly be argued, however, that the latter is partly dependent on the former, since the downdraught of a wave system would inhibit cloud formation and the updraught would assist it.)

Cloud-street wave flow was first experienced in Germany during the early 1960s (Jaeckisch 1970) and, although the phenomenon has since been experienced elsewhere (Kuettner 1972), there is no known record of it in the British Isles. (Bradbury (1973) discusses a possible occurrence but this was associated with a line of vigorous cumulonimbus, not an organized system of cloud streets in which the clouds

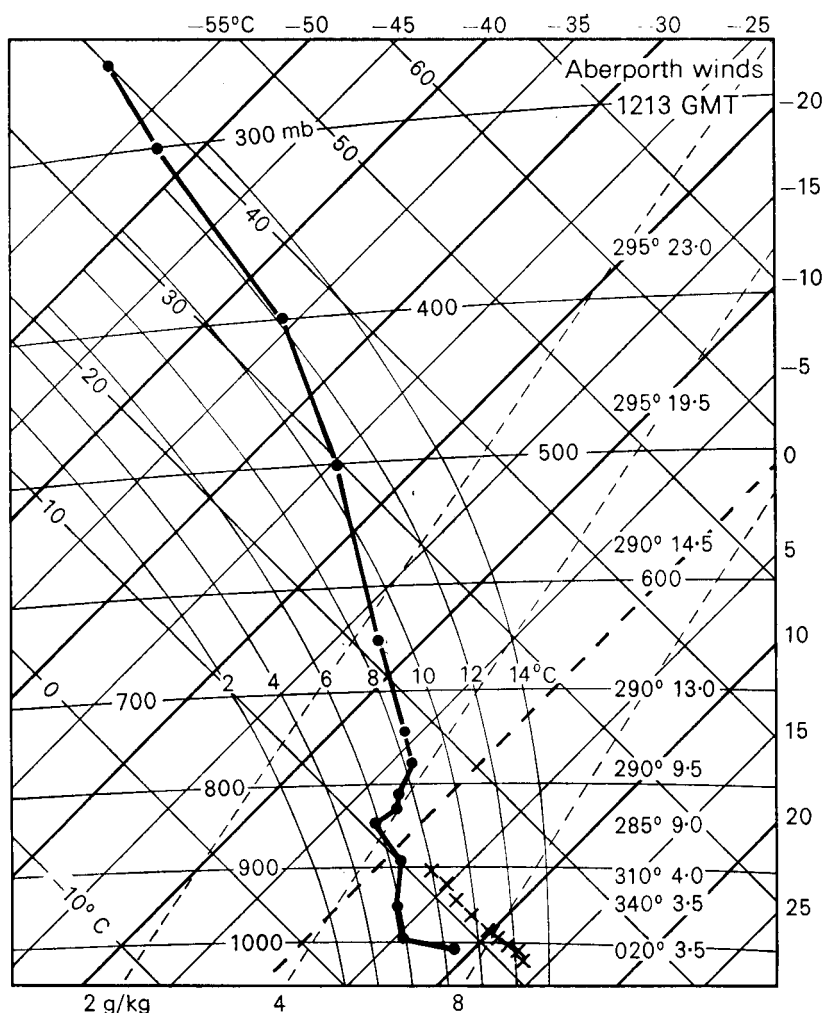


Figure 8. Aberporth and Cardington ascents during the afternoon of 9 May 1979.

●—● Aberporth 1213 GMT (winds in  $\text{m s}^{-1}$ ).  
 ×—× Cardington 1455 GMT.

were of limited vertical development.) Compared with lift in inversion and cumulus wave flows, lift in cloud-street wave flow can extend to considerable heights above cloud tops (some 1800 m in one early report (Jaekisch 1970)) and the rate of lift normally seems to be about  $2 \text{ m s}^{-1}$ .

Following Scorer (1953) and using a scale devised by Wallington (1953) it can be shown that on this occasion the wavelength between 1800 m and 2300 m was probably very close to the cloud-street spacing of 5–6 km as measured from the satellite photograph. Between 2300 m and 2700 m the wavelength increases to 9–10 km, so it is unlikely that the lift encountered by the K-13 glider would have extended much higher.



Because of the vertical extent and strength of lift on 9 May two types of wave flow, inversion and cumulus wave flow, can quickly be eliminated. The comments by some pilots that the stratocumulus west of Little Rissington was lenticular, together with the visual evidence of the satellite photograph, do, however, support the idea that the waves were orographically induced. Although the easternmost cloud element of the 'wave train' extending from Radnor Forest is approximately 20 km west-north-west of Little Rissington it is conceivable that wave motion could continue in clear air beyond this point.

However, for orographic waves to develop it is essential that the wind should blow across a ridge, in this case from about  $290^\circ$ . Unfortunately, a lack of synoptic data makes it difficult to ascertain the true wind flow over central Wales on 9 May. On the one hand, assuming the wind flow at 400 m (taken as the average altitude of Radnor Forest) closely follows the isobaric pattern, then the flow was between north and north-east from about 06 GMT that morning. Supporting this argument, the observer at Trecastle, some 50 km to the south-west but only 312 m above sea level, recorded the wind at  $050^\circ$ ,  $1 \text{ m s}^{-1}$  at 15 GMT, while 100 km to the west of Radnor Forest at Aberporth the 12 GMT wind at 600 m above sea level was  $340^\circ$ ,  $3.5 \text{ m s}^{-1}$ . Above 600 m the wind backed to  $280^\circ$ – $290^\circ$  then remained constant in direction above 1200 m (Table III).

**Table III.** *Aberporth radar winds, 9 May 1979, 06 and 12 GMT*

Heights in metres above sea level, speeds in metres per second					
	Height	06 GMT		12 GMT	
		Direction	Speed	Direction	Speed
Surface	132	360	3.0	020	3.5
	500	005	5.0	—	—
	600	—	—	340	3.5
	900	360	5.0	310	4.0
	1200	—	—	295	5.5
	1500	295	8.0	285	9.0
	1800	—	—	285	7.0
	2000	280	10.0	—	—
	2100	—	—	290	9.5

On the other hand, as much of central Wales lies above 400 m above sea level it could be argued that the surface wind can be estimated from the 1300 m wind in much the same way that Findlater *et al.* (1966) relate the surface wind near sea level to that at 900 m. If this is a valid argument then the wind over Radnor Forest could well have been  $280^\circ$ – $290^\circ$ ,  $4 \text{ m s}^{-1}$ . Therefore, with a slightly stronger wind at ridge levels (maximum elevation 660 m above sea level), it is possible that lee waves could have developed. Even so the theoretical maximum vertical velocity would have been only  $1 \text{ m s}^{-1}$  at 1500 m (after Casswell 1966), half the observed rate of lift, and this would have occurred close to the source of the wave.

Nicholls (1973), for example, describes a well-defined mountain wave system originating over the Black Mountains in a south-westerly airstream. Even though the wind at 600 m above sea level was  $10 \text{ m s}^{-1}$  and the maximum vertical velocity measured as  $2.5 \text{ m s}^{-1}$  the wave motion was quickly damped down some 50 km from the point of origin, although minor waves persisted beyond this point. Therefore, even if a west-north-westerly wind had existed across Radnor Forest on 9 May, it is unlikely that the wind would have been strong enough to maintain a wave train, in which the observed vertical velocity was greater than the theoretical maximum vertical velocity, some 85 km or so from its source.

Thus, although there must be considerable doubt whether mountain waves could have existed, all the conditions required for cloud-street wave flow were present—the weak surface flow, upper winds blowing across the cloud streets and a cloud-street spacing similar to the wavelength of the air above the inversion.

### Conclusion

Undoubtedly wave flow which was not of the usual type was present on 9 May 1979. Although mountain waves can exist above a cumulus field (Bradbury 1963) the wind regime on this occasion was not really suitable for the development of marked mountain waves and, on balance, the evidence points to another sort of wave flow—cloud-street wave flow.

Since a powered glider would have been available to make a comprehensive survey of the area, it must remain a matter of regret that, as forecaster on the day in question, I did not immediately recognize the significance of the competitors' comments about wave flow. Like most forecasters and glider pilots I was conditioned to think of only one form of wave flow—mountain wave flow—which is not really surprising considering the lack of comments on any other form of wave flow in British publications.

The conditions under which this particular event occurred are not very unusual and it may well be that cloud-street wave flow is a more frequent phenomenon than its absence from the literature would suggest.

### Acknowledgements

I should like to thank all those competitors at the 1979 Inter-Services Gliding Championships who were good enough to describe their experiences (to an initially disbelieving forecaster) and especially Barry Elliot, Air Commodore Cooke and Sergeant Hancock. I am also grateful to the University of Dundee for permission to publish their satellite photograph.

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## **World surface climatological data—methods of quality control and archiving**

By J. N. Ricketts

(Meteorological Office, Bracknell)

### **Summary**

A detailed description is given of a quality control scheme developed for use with world CLIMAT data. Manual and computer-based methods are used, in each case where they can be applied most effectively. There is also a considerable degree of checking between the various methods.

The general quality of transmitted CLIMAT data is found to be not good and it is hoped that, as the World Climate Programme gathers momentum, there may be a definite improvement.

### **Introduction**

The study of climate and climatic change has its basis largely in instrumental records, particularly of pressure, temperature and rainfall. The averages or totals of the numerical values of these variables over a calendar month have been standard climatological parameters for almost as long as the instrumental records themselves. It is on the evidence of such data that hypotheses on climatic change will be tested.

Over the years, as nearly every country has come to realize the importance of climatic data, a global network of climatological stations has been developed. There are now about 1500 stations in the world which are scheduled to report each month. A considerable amount of work has been and is being done to extend regional data series back in time. Much of this work involves searching through old diaries, parish registers and the like. Less effort seems to have been put into the problems of maintenance and quality control of data series in real time; this has been the task of a small team in the Synoptic Climatology Branch of the Meteorological Office for a number of years, although the detailed checking procedures have been developed only recently.

Bryant (1979) has already given a detailed description of the method used in the examination and storage of climatological data from stations in the United Kingdom (plus a few overseas). There are some similarities in the methods described here, mainly in the manner of transferring data from written records to magnetic storage.

### **Collection of data**

Surface and upper-air climatological data are received at Bracknell by the Global Telecommunication System (GTS) and may arrive either as teleprinter hard copy or as output retrieved from a data set which is continuously updated. The messages are not decoded before they are entered into the data set and the only automatic sorting is on output when surface, upper-air and the other types of climatological message are printed separately.

Most of the data arrive within a fortnight or so after the end of each month. Extraction is normally from teleprinter copy and the data are entered into 5-year data books under the appropriate headings—pressure, temperature, vapour pressure and rainfall (including number of rain days and the appropriate 'quintile'\* for the month). Hours of sunshine and percentage of normal sunshine are currently entered

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\* The use by meteorologists of the word 'quintile' to denote ranges of a variable such that each range contains one fifth of the total number of observations is anomalous and not acceptable to statisticians. The quintiles are normally defined to be the four boundaries between such ranges.

in a separate book, but will be included in the same book as the other parameters from 1981. Whenever possible, departures from normal are calculated at the time of entry and are later entered on northern hemisphere charts.

A few countries send confirmation copies which start arriving about mid-month. These are particularly useful since they are free from transmission errors and may have been compiled with more care than the teleprinter messages.

### **Initial quality control**

Entry into books provides immediate comparison with previous data (up to four years) which is a useful first step in the quality control procedure. Plotted charts of monthly mean sea-level pressure, temperature, total rainfall and sunshine provide spatial quality control. The surface-pressure chart is analysed in the normal way using actual values but the others are analysed on the basis of departures from normal, providing a much more coherent field than the actual values. Inevitably, in many areas the average distance between stations is too great for anything but very rough intercomparisons of means and anomalies to be made. However, large apparent inconsistencies are easily recognizable and can be checked without much difficulty. Where there is a good density of stations doubtful values stand out clearly.

### **Keying to magnetic tape**

About three weeks from the beginning of each month, when most of the data have been logged and checked, keying forms are completed and sent to the Processor-controlled Keying (PCK) section (Figure 1).

Each form is headed with a 5-figure month/year group which is automatically added to the start of each record as it is keyed. The rest of the record is very similar to the original CLIMAT message but with two differences: there is only one field for pressure data, normally occupied by sea-level values, but by station pressure for high-altitude stations; and at the end of each record a single number is allocated to a flag value which indicates the relative position of any doubtful element. If two or more elements are doubtful a flag value of zero (0) is used.

The keying process involves each element being assigned a field. If it has not been reported, it is skipped and a 'missing' value of — 32768 is automatically entered. Simple checks are also included: for instance the maximum allowable number of rain days is 31 and the quintile value cannot exceed 6. These checks both detect incorrect fields caused by errors in field-skipping and provide a further test on the entered values. The most effective keying check, however, is provided by the verification process described by Bryant (1979) in which all the data are keyed a second time and automatically checked against the values originally entered.

### **Further quality control (automatic)**

Once the keying process has been completed and the data are on magnetic tape, a working copy is made. The PCK tape is retained as a back-up until the processed data have been written to the final archive tape. A series of programs is run which check, correct and sort the data. The first program does the checking and provides an output of exactly what has been entered. The tests in detail are as follows:

*Pressure.* The abbreviation of sea-level pressure values (reported in tenths of a millibar) precludes any numerical value greater than 0999. The minimum station-level pressure so far encountered is greater than 600 mb, so any value between 1000 and 5999 is assumed to be erroneous. Such errors are normally the result of incorrect field-skipping.

MONTH		YEAR		
1	2	9	7	9

NUMBER			PRESSURE (mb) M.S.L. (but S/F for high-level stations)	TEMPERATURE (°C)  Minus = 1	VAPOUR PRESSURE (mb)	RAINFALL (trace = 9999)			SUNSHINE			FLAG
Block	Station					Rain days ≥ 1 mm	Amount (mm.)		Quintile	Amount (hrs)	% ÷ 5 of normal	
4	1	715	180	172	114			75				
		739		127	77	2		154				
		756	170	205	170	13		39				0
		765	180	200	113	3		34				5
		768	170	181	117	0		0				
		780	170	197	115							
4	2	027	846									
												14
						3		175	2	8	7	19
				242	183	0		04				
		133	149	223	141	0		04				
4	3	041	158	192	152	2		115				
		057	135	267	241	0		04	2	8	9	20
		063	143	212	157	0		04	3	0	0	21
DO NOT PUNCH THIS SECTION →			1	2	3	4	5	6	7	8		

Figure 1. Example of keying form from which CLIMAT data are keyed to tape.

Block numbers are repeated on the tape until a new block number is encountered.

41756 vapour pressure and rain days were considered doubtful—suspect transmitting error in this case.

41765 rainfall doubtful.

Sunshine percentages of normal as coded—multiply by 5 to obtain value.

*Temperature* is effectively tested twice. First, values outside the range  $-65^{\circ}\text{C}$  to  $+40^{\circ}\text{C}$  are excluded. (Vostok base in Antarctica frequently reports monthly means below  $-65^{\circ}\text{C}$ , but for any other stations such values would almost certainly be erroneous.) The second test uses the relationship between saturation vapour pressure  $e_s$  and temperature  $T$ . The relationship used here has been derived from observation and provides a more rigorous check than simply using the empirical  $e_s/T$  equation.

$$\ln e \leq 0.006 \times T + 4.23,$$

where  $e$  is vapour pressure ( $\text{mb} \times 10$ ) and  $T$  is temperature ( $^{\circ}\text{C} \times 10$ ). Queries raised by this test are normally the result of doubtful vapour pressure values (except when temperature has already been flagged), since no other check is applied to this element.

*Rainfall* and *rain days* are tested for consistency. A rain day is defined as one in which 1 mm or more of rain has fallen; thus the number of rain days cannot numerically exceed the rainfall. Both rain days and quintile have already been checked during the keying process and an upper limit cannot be specified for rainfall.

Finally, values of *sunshine duration* in absolute terms and as a percentage of normal are tested individually and together. The theoretical maximum amount of sunshine in a 30-day month is 720 hours in high latitudes in summer and this value cannot be exceeded. It is rare for sunshine to exceed 250 per cent of normal, so an upper limit of 300 per cent is specified.

Only very recently a comprehensive set of confidence limits has been developed. They were calculated using the World Historical station data sets, which are largely based on World Weather Records and were created within the Synoptic Climatology Branch from tapes supplied by the National Center for Atmospheric Research, Asheville, North Carolina. For temperature and pressure data the confidence limits are defined as:

$$\bar{x} \pm 3\sigma_{n-1},$$

where  $\bar{x}$  is the mean and  $\sigma_{n-1}$  is the standard deviation of values in the normal period. For rainfall data they are:

$$\begin{aligned} &\bar{x} + 1.5(x_{G-1} - \bar{x}) \\ \text{and } &\bar{x} - 1.5(\bar{x} - x_{L+1}), \end{aligned}$$

where  $x_{G-1}$  and  $x_{L+1}$  are respectively the next highest and next lowest rainfall totals during the normal period. Should the expression give a negative lower confidence limit, it is set to zero.

Not all keying errors or omissions in the manual quality control procedures can be detected by the above checks. Scrutiny of the keyed data is also necessary, with reference to data books and plotted charts for chronological and spatial comparisons. None the less, automated checking is a useful component of the quality control system.

### Corrections and final checks

Corrections and additional flags shown to be necessary by the above checks are incorporated before ordering the data according to World Meteorological Organization (WMO) block/station number. These numbers are themselves checked by reference to a list of stations from which records have been received during the previous three years. Any reported station numbers not on the standard list are printed out, as are the numbers of stations from which data have not been received. The former list is more important since it represents either a keying error or occasionally a new station, which would be added to the standard station list after reports for three consecutive months have been received. There is also a test for duplicate station numbers, which normally result from a report being entered twice.

Additional corrections shown to be necessary at this stage necessitate repeating the procedures described in this paragraph.

### Late data and further checking of flagged records

Occasionally, late reports arrive by teleprinter in the form of 'retard' messages, but it is necessary to wait for the relevant issue of *Monthly climatic data for the world\** (MCDW) for most missing data. This

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\* MCDW is produced by the US Department of Commerce and is published by the National Oceanic and Atmospheric Administration (NOAA) with sponsorship from WMO.

also provides a useful opportunity for checking those values which have been flagged. These late data and corrections are entered on punching forms in the same way as are the 'real-time' data, even when a record is being corrected. The subsequent routine of quality control, keying and checking is as already described and the data are finally written to a 'late data' tape.

Once all available data for a year have been written to tape, they are added to the World Historical data sets of pressure, temperature and rainfall. (This is some months after the end of the year in question since the aim is to include as many data as possible.) Flagged elements are assumed to be missing for the purposes of archiving, but a list is kept so that they may eventually be obtained from other sources, such as national meteorological yearbooks.

### Problems and comments

The most obvious and numerous problems are a result of coding and transmission errors. Whereas SYNOP and other frequently used codes are well understood by users, there is considerably less familiarity with the CLIMAT code and, despite clearly defined rules, a wide variety of non-standard headings and message formats are received. These normally present no problem to the human eye but there are considerable complications in designing a system of message handling by computer. Furthermore, savings in staff time are unlikely to be significant. The only automatic sorting attempted is between surface, upper-air and other types of message. Even then, the basic indicator (CS, CU, CE, etc.) is sometimes omitted, causing the subsequent message to be output in the same stream as the previous message. For these reasons, it has been decided not to automate message handling any further.

The time of receipt of climatological messages, as opposed to synoptic data, is relatively unimportant. Quality is of far greater concern and unfortunately the general quality of the message contents is not good. For instance, there is a WMO requirement for sunshine data to be added to CLIMAT reports (World Meteorological Organization 1972), yet some countries never include it. Rainfall is occasionally reported in millimetres and tenths rather than whole millimetres and there appears to be confusion over the definitions of rain days and quintiles. A day of rain, as defined for the CLIMAT report, is one in which 1 mm or more of rain has fallen (World Meteorological Organization 1972). However, regional practices are such that more than one definition may exist. In the Meteorological Office, for example, a 'rain day' is defined as having 0.2 mm or more of rainfall, whereas a 'wet day' has 1 mm or more. It is probable that some countries use the former definition, or a local equivalent, in their CLIMAT reports.

In regions or seasons of plentiful rainfall, quintile boundaries are based on equal divisions of the frequency distribution during the normal period. Quintile values 0 and 6 denote rainfall totals respectively less than and exceeding any value in the normal period. Where rainfall is sparse, the quintile is a description of the frequency of months without rain; the more dry months there are, the higher is the quintile. In such cases the quintile is of little value, but even when the quintile and rainfall may be expected to have some relationship it is frequently not evident.

There are times when CLIMAT messages appear to have been hastily compiled and transmitted, perhaps because they are an extra component in an already tight work or transmission schedule. Confirmation copies sent by post are of considerable assistance and it would be helpful if they were made the rule rather than the exception. Much of the data published in *Monthly climatic data for the world* is based on telecommunication reports. Unfortunately, there has been little quality control and it is frequently not possible to correct doubtful values from this source.

### **Changes of site and instrumentation**

The effects of site and instrument changes are potentially the most serious from the climatological viewpoint. If the stations are closed, resited or re-equipped, the effect on the climate record may easily be mistaken for a true variation in climate. Past data are full of 'pseudo-variations' of this sort which require detailed station histories to resolve them. This would often involve reference to the country of origin for the necessary information.

### **Acknowledgements**

The undoubted value of the climatological archive owes much to the dedication and consistent work of the 'CLIMAT Lab' staff in the Synoptic Climatology Branch of the Meteorological Office. To them, both past and present, I can only offer my thanks.

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### **Mrs Dorothy Groves**

We record with great regret the death on 30 June 1980 of Mrs Dorothy Groves, widow of the late Major K. G. Groves, O.B.E., J.P., M.A., LL.M.\* Mrs Groves, daughter of the American foreign editor of the *Daily Express*, was co-founder with her husband of the L. G. Groves Memorial Prizes and Awards† and attended every presentation ceremony with him from 1946 to 1977. Quieter and more retiring than Major Groves though she was, her charm and grace of manner added much to the atmosphere of a unique ceremony, and in 1976 she herself presented the Meteorological Observer's Award.

### **Notes and news**

#### **Retirement of Mr D. McNaughton**

Mr D. McNaughton, Assistant Director (Telecommunications), retired from the Meteorological Office on 9 October 1980 after a career of 42 years.

Don McNaughton was educated in Coatbridge, Lanarkshire and joined the Office in 1938 as an Assistant III. In 1941 he went to Canada where he became involved in the meteorological aspects of the Air Training scheme and later underwent forecaster training at Toronto University. He was promoted to Assistant II in 1943 and became a Flying Officer in the RAFVR the following year when he returned to the United Kingdom. In 1945, after retraining for radiosonde work at Downham Market, he went first to Lerwick (where he was demobilized in 1946) and then to the Falkland Islands as the Experimental Officer in charge of the Radiosonde Unit. He was subsequently seconded to the newly created meteorological service of the Falkland Islands Dependencies Survey in 1951 and remained there for a further four years, during which time he made his début into the communications field as Director of the local broadcasting service.

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\* *Meteorol Mag*, 1979, 108, 316.

† *Meteorol Mag*, 1975, 104, 57.

After his return to the United Kingdom in 1955 he received further training in forecasting duties and went to London/Heathrow Airport where he was promoted to Senior Experimental Officer in 1957. Two years later he took charge of the newly opened Glasgow Weather Centre and remained there for the next seven years, showing great enthusiasm for pioneering new areas of public service work, giving numerous talks and presentations on radio and television and clearly enjoying the wider outlets for his organizational flair. On promotion to Chief Experimental Officer in 1967 he returned to the civil aviation field and subsequently 'shuttled' between London Airport and the parent Headquarters Branch at Bracknell for the following five years.

He finally transferred to the Telecommunications Branch in 1972, at a time when the Office's first message-switching computer system was entering service, and took charge of the Meteorological Telecommunication Centre two years later when the main functions of the Centre became computerized. Mr McNaughton was appointed Head of Branch in 1976 and has spent the past four hectic years devising means of coping with the ever-increasing volume of data passing through the Centre, and with the constant demands of users throughout the Office for more and more telecommunication facilities. He has met these relentless pressures with good humour and steady application, inundating his colleagues with an eloquent outpouring of prose on every topic within his purview. He is widely known and respected for his contributions to international planning and collaboration in the telecommunication field.

Don McNaughton does not have any immediate plans to move house, or to substitute any one predominant activity for that which he leaves behind with some regret, but we wish him and Mrs McNaughton a long and happy retirement and a personal microclimate providing anomalously low pollen counts.

M. J. Blackwell

### Investigations of dust cloud from Mount St Helens volcano

Following the eruption of the Mount St Helens volcano on 18 May, which created great public interest, forecast trajectories were calculated on 21 May and the following two days for two levels in the upper troposphere; they indicated that the main dust cloud in the troposphere would reach North Africa on 23 or 24 May. At short notice, the Hercules aircraft of the Meteorological Research Flight was detached to Gibraltar on 23 May to study the passage of the plume. A dust cloud was successfully sampled on 24, 25 and 26 May off the north-west coast of Africa between 26 000 and 31 000 feet. In addition, the Canberra aircraft was used to take samples of dust in the stratosphere over and near the United Kingdom. At the time of writing (August) the analysis of the samples is not complete.

### 100 years ago

The following extract is taken from *Symons's Monthly Meteorological Magazine*, November 1880, 15, 156-157:

#### WILL-WI'-THE-WISP.

*To the Editor of the Meteorological Magazine.*

SIR,—I think you said in the Magazine some time ago that you had never met with, in the flesh, the man who had seen the *ignis fatuus*, or Will-wi'-the-Wisp. I beg to enclose a paragraph which I cut out of the *Fife News* newspaper some weeks since, and you will see from it that it has been seen there. I can also tell you that in 1832, when a boy herding cows on the farm of Crosshouses, near Kingskettle.

Fifeshire, that I had frequent opportunities of seeing Will-wi'-the-Wisp, or "Spunkie," as it is called in the old Doric.

On the farm of Crosshouses was a bog, about 300 yards from the door of the dwelling-house, and it was in the bog—or rather over the bog—that I saw the light moving about. The best description I can give of it is that it resembled that of a lantern being carried about by a person for some twenty or thirty yards. Sometimes I had seen it move zig-zagly about these distances, and sometimes in a straight line, and always at the same rate of speed—that of a person's usual speed when walking.

My grandfather frequently saw it. Sometimes, when coming in from suppering the horses, he would cry in haste to come out and see "Spunkie" in Pilkim Moss, when I, and the others at the fireside sitting, would run to the door to see it. When all of us were either less or more afraid of the wandering light, many times I have gone to bed at once—*after prayers though*—and buried my head among the bed-clothes that "Spunkie" might not get hold of me; for every person regarded the phenomenon with dread.

I also saw it in a bog called Powglass, on the farm of Dawnfield, close by Crosshouses, several times in the same year, 1832. I have lived since, as a gardener, in four counties in England, and four in Scotland, but never saw "Spunkie" in either of them. I must say, however, that no bogs—or rather quagmires, for Powglass bog partook more of the quagmire—were in close enough proximity to where I lived, in those eight counties, to give me a chance of seeing Will-wi'-the-Wisp again.

Powglass bog and Pilkim bog have both been drained, and "Spunkie" is no longer seen there.

I have one or two cousins still living in the parish of Kettle, who, I am certain, will vouch for the truth of what I have stated to you.

Regarding the paragraph enclosed, I think I can procure the address of the writer of it, on application to the office of the *Fife News*, Cupar, Fife.

DAVID ELDER, Gardener.

*Silverbut Hall, Hawick, June 21st, 1880.*

# "STAR.

"WILL-WI'-THE-WISP.—Under the rapid march of improvements in draining, Jack-wi'-the Lantern, or Will-wi'-the-Wisp, has of late years been rarely seen in this quarter. One dark evening last week, however, the fiery imp was all activity in the locality of the peat moss, to the west of this, and created some excitement by his 'flitting, flaunting flame.' Decayed vegetable or animal matter, it is well known, is the cause of this light. In damp churchyards, in the olden time, where dead bodies were not buried far under the surface, such lights poured forth, and among our forefathers were familiarly known as 'dead lights.' In a dark age, and while such lights were so frequently cast forth, it is scarcely to be wondered at that superstitious beliefs were closely associated with such appearances."—*Fife News*.

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\* \* This phenomenon, known variously as *ignis fatuus*, jack-o'-lantern, will-o'-the-wisp, corpse candles, etc. is a form of chemical luminescence produced by the combustion—usually spontaneous—of mixtures of gases released by rotting vegetation. The gases involved include methane, phosphine and hydrogen sulphide. Combustion takes place at low temperatures, little if at all above that of the ambient air; the 'flames' may flicker just above the ground or surface of a bog, or may float along in a light breeze.

### Obituary

We record with regret the death on 22 April 1980 of Mr B. A. Halls, Assistant Scientific Officer, who was at the time on secondment to WMO in Geneva where he was employed in monitoring the operation of the World Weather Watch. Mr Halls joined the Office in 1964 as a specialist teleprinter operator in the Telecommunications Branch and transferred to the Scientific class in 1966. He was seconded to WMO in November 1976.

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We record with regret the death on 26 May 1980 of Mr S. A. Darke, Assistant Scientific Officer, Cardiff/Wales Airport. Mr Darke joined the Office in October 1978.

### Official publication

The following publication has recently been issued:

*Solar radiation data for the United Kingdom 1951–75.* Bracknell, Meteorological Office, 1980.

This 110-page publication summarizes solar radiation data from both Meteorological Office and co-operating stations for the period 1951–75, providing a useful reference for meteorological, building and agricultural research and for commercial solar-heating organizations.

After an introduction describing the radiation instruments used, the following four sets of tables are presented:

(a) Frequency distributions of hourly irradiances for 11 stations for each month for the period 1966–75.

(b) As (a) for daily irradiances.

(c) Monthly irradiation totals available from 26 stations for the 25-year period 1951–75.

(d) Numbers of consecutive days with irradiation above and below given thresholds for 6 stations.

Additionally, monthly maps of mean daily global irradiation (Cowley, J. P., 1978, *Meteorol Mag*, 107, 357–373) are reproduced.

### Publication received

The following publication has been received:

*Syllogeus No. 26, Climatic change in Canada*, edited by C. R. Harington. Ottawa, National Museums of Canada, 1980.

This issue consists of a project on climatic change in Canada during the past 20 000 years and contains five discussions by different experts in the field. It is hoped that they will animate research and policy-making groups in research institutions throughout Canada. A copy is held in the National Meteorological Library, Bracknell.



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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'.

The responsibility for facts and opinions expressed in the signed articles and letters published in this magazine rests with their respective authors.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd 698260 K15 11/80

Annual subscription £21.18 including postage  
ISBN 0 11 722067 1  
ISSN 0026-1149



# THE METEOROLOGICAL MAGAZINE

HER MAJESTY'S  
STATIONERY  
OFFICE

December 1980

Met.O. 931 No. 1301 Vol. 109









# THE METEOROLOGICAL MAGAZINE

No. 1301, December 1980, Vol. 109

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551.509.616

## **A review of three long-term cloud-seeding experiments**

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### **Summary**

The results of three long-term, randomized cloud-seeding experiments, carried out in Tasmania, Florida and Israel, are analysed and assessed. All three experiments carry claims to have increased the precipitation reaching the ground by an average of more than 15 per cent, statistically significant at the 5 per cent level. The present analysis does not support these claims for the Tasmania and Florida experiments. In the case of the Israeli experiment the statistical evidence for a positive seeding effect is strong, but needs to be reinforced and interpreted in terms of the observed microphysical and dynamical evolution of the clouds. Since this is one of the very few well-designed experiments that has produced consistently positive results over several years it is important to determine why the Israeli cloud systems should be more responsive to seeding than apparently similar clouds in other parts of the world.

### **1. Introduction**

After 30 years of effort involving some hundreds of projects in many countries, and despite many confident claims and optimistic official reports, there is in fact very little convincing evidence that it is possible, by seeding clouds with artificial nuclei, to induce economically significant increases in precipitation that can be distinguished from the natural variations of rainfall. This unsatisfactory state of affairs has been recognized by the World Meteorological Organization which is investigating the possibility of establishing a long-term international experiment with a good chance of producing a definite, statistically significant result that will command the confidence of the world meteorological community and form the basis for judging the potential and cost-effectiveness of such projects, especially in semi-arid regions.

Although many cloud-seeding operations have not been designed as scientific experiments and are therefore not susceptible to proper scientific analysis and evaluation, a number of responsible long-term national experiments have been conducted in recent years, all based on statistical designs incorporating randomization between target and control areas and/or seeded and unseeded periods and employing similar techniques of statistical evaluation including tests of significance.

In this study I review, assess and compare the results of three recent major long-term experiments carried out in widely separated parts of the world—Tasmania, Florida and Israel. All three experiments have several important features in common. They were all directed by highly reputable scientists very

experienced in the fields of cloud physics and cloud seeding. They all commanded considerable resources. They were all randomized experiments, operated over at least five years, and were designed and evaluated with the help of highly competent statisticians. They were all concerned with the seeding of moderately supercooled, mostly convective clouds with silver iodide dispersed from aircraft. They carry claims to have increased the precipitation reaching the ground by an average of more than 15 per cent, statistically significant at the 5 per cent level, although, in the Tasmania experiment, this was made only for the autumn season.

My analysis does not find strong support for these claims except in the case of the Israeli experiment where the statistical evidence is strong but will need to be reinforced and interpreted in terms of the observed microphysical and dynamical evolution of the clouds.

## 2. The Tasmania experiment (1964–70)

**2.1. Objective.** The objective of the Tasmania experiment (Smith *et al.* 1977, 1979) was to determine whether seeding cloud systems with silver iodide smoke released from an aircraft could increase the precipitation in a hydroelectric catchment area in Tasmania and to determine the circumstances under which such increases could be achieved.

**2.2. Experimental plan.** The experiment was conducted over the six-year period 1964–70 but seeding was carried out only on even-dated years in order to minimize any persistent effects, i.e. the carry-over of seeding effects from one year to the next. The experimental years were divided into periods of about 12 days and consecutive periods were arranged in pairs, one member of each pair being selected for seeding at random, the other member being used as an unseeded control period.

The target area of about 1000 mile<sup>2</sup>, located on a plateau, 2500–3000 ft above sea level, in the centre of Tasmania, has a mean annual rainfall of about 40 in and was equipped with 16 rain-gauges read daily. The rainfall in the target area, mainly associated with the frontal systems of mobile depressions is fairly evenly distributed through the seasons, about 10 per cent of the total falling as snow in winter.

There were three control areas, one to the north (NC), one to the south (SC) and one to the north-west (NWC) of the target and these were used in various combinations in the analysis of the results. The NC area had a mean annual rainfall of 45 in and 18 rain-gauges, the SC area a mean annual rainfall of 35 in and 12 rain-gauges and the NWC area a mean annual rainfall of 55 in and 8 rain-gauges.

Assessment of the seeding operation took the form of a comparison between the rainfalls in the target area during the seeded periods with estimates of the rain that would have fallen in this area in the absence of seeding. The latter was estimated by correlation with rainfalls in the control regions measured in both seeded and unseeded periods, the correlation coefficients between historical values of the rainfalls in the target and control areas exceeding 0.9.

Seeding took place only when the clouds were judged suitable, i.e. if their tops were colder than  $-5^{\circ}\text{C}$  for stratiform clouds and only when the cloud depth exceeded half of the terrain clearance. Additionally cumuliform clouds were deemed suitable for seeding only if they were compact and solid in appearance with tops vertically above their bases indicating the absence of strong vertical wind shear. Seeding was carried out at cloud base wherever possible and upwind of the target area, allowing time for the clouds to drift over the target before precipitating.

The effects of seeding were judged not just in terms of the ratio  $T_s/T_u$  of the rain falling in the target area during the seeded and unseeded periods as in FACE (see section 3) but rather in terms of the double ratio  $(T_s/T_u)/(C_s/C_u)$ , where  $C_s$  and  $C_u$  refer to the rain falling in the control areas during the seeded and unseeded periods. Use of this double ratio is intended to compensate for the fact that the rainfall everywhere (in both T and C areas) may turn out to be either abnormally high or abnormally

low in the seeded periods compared with the unseeded periods relative to the long-term averages, and therefore to make some allowance for conditions during the operating period not being representative. For example, if  $T_s/T_u$  turns out to be  $< 1$ , this might be interpreted in terms of seeding producing an actual decrease in rainfall but if  $C_s/C_u$  is also  $< 1$  this might imply that the rainfall was naturally low everywhere, in both target and control areas, during the seeded periods compared with the unseeded periods even though these were designated at random and interleaved; in this case use of the double ratio would help correct for this and it is now widely used in weather-modification experiments including the Israeli experiment reviewed later.

2.3. *Assessment of the results.* Table I shows that the total rainfall in the target area during the 54 seeded periods of the 4-year trial was less than that in the 54 unseeded periods, the ratio being 0.92. Whilst more rain fell in the target area during the seeded than in the unseeded periods in autumn and spring, the contrary was the case in the winter and summer seasons. When allowance is made for unequal numbers of seeded and unseeded periods, making comparisons on the basis of the average rainfall per seeded or unseeded period, the ratios of seeded to unseeded rainfalls are autumn (0.84), spring (1.28), winter (0.81), and summer (0.68). On this basis one might conclude that seeding led to a decrease in the target-area rainfall over the four years as a whole; indeed only 19 out of 54 seeded periods produced more than the average value for the 108 operational periods. One might also infer that seeding produced positive results only in spring.

Table I. Tasmania—rainfall analysis for seeded years

										Double ratios = $\frac{T_s/T_u}{C_s/C_u}$		
		<i>n</i>	<i>T</i> inches	<i>C</i> <sub>1</sub> inches	<i>C</i> <sub>2</sub> inches	<i>T</i> / <i>C</i> <sub>1</sub>	<i>T</i> / <i>C</i> <sub>2</sub>	<i>T</i> / $\frac{1}{2}(C_1 + C_2)$	<i>T</i> <sub>s</sub> / <i>T</i> <sub>u</sub> *	<i>C</i> = <i>C</i> <sub>1</sub>	<i>σ</i>	<i>C</i> = $\frac{1}{2}(C_1 + C_2)$
Totals all four years	<i>S</i>	54	84.76	85.84	66.95	0.99	1.27	1.11	0.92	1.04		1.06
	<i>U</i>	54	92.15	97.47	79.31	0.95	1.16	1.04				
Autumn	<i>S</i>	14	20.46	19.06	14.47	1.07	1.41	1.22	0.84	1.25	0.05	1.25
	<i>U</i>	11	19.21	22.43	17.36	0.86	1.11	0.97				
Winter	<i>S</i>	14	22.03	26.11	19.82	0.84	1.11	0.96	0.81	1.02	0.5	1.07
	<i>U</i>	14	27.28	33.11	27.46	0.82	0.99	0.90				
Spring	<i>S</i>	16	30.67	30.09	22.99	1.02	1.33	1.16	1.28	0.98	0.6	0.99
	<i>U</i>	18	27.02	25.99	20.40	1.04	1.32	1.17				
Summer	<i>S</i>	10	11.60	10.58	9.67	1.10	1.20	1.15	0.68	0.94	0.8	0.93
	<i>U</i>	11	18.64	15.94	14.09	1.17	1.32	1.24				

*T* = rainfall over target area. *C*<sub>1</sub> = rainfall over control area 1 (= (NWC + SC)/2).

*C*<sub>2</sub> = rainfall over control area 2 (= (NC + SC)/2).

$\sigma$  = statistical significance level for *C* = *C*<sub>1</sub>.

*S* = seeded period. *U* = unseeded period. *n* = number of periods.

\* calculated per period.

However, the picture changes markedly when comparisons are made with the rainfall in the control areas. Computation of the double-ratios ( $T_s/T_u$ )/( $C_s/C_u$ ) suggest that seeding was most effective in autumn with an indicated increase of 25 per cent significant at the 5 per cent level, there being no significant increase in any other season or over the years as a whole. Moreover, in the intermediate years during which no seeding took place (Table II), the total rainfall in the target area was equal to the average totals for the three control areas but, in autumn, the target area rainfall was higher than the control value by 8 per cent. If we now use these unseeded years instead of the unseeded periods in the seeded years as a control, the double ratio indicates an increase of only 13 per cent due to seeding in autumn.

**Table II.** *Tasmania—rainfall analysis for unseeded years*

	<i>T</i> <i>inches</i>	<i>C</i> <sub>1</sub> <i>inches</i>	<i>C</i> <sub>2</sub> <i>inches</i>	<i>T/C</i> <sub>1</sub>	<i>T/C</i> <sub>2</sub>	<i>T</i> / $\frac{1}{2}(\text{C}_1 + \text{C}_2)$
Totals all three years	99.37	111.53	87.85	0.89	1.13	1.00
Autumns only	31.57	32.01	26.24	0.97	1.20	1.08

*T*, *C*<sub>1</sub> and *C*<sub>2</sub> as defined in Table I.

These calculations demonstrate how one can arrive at quite different conclusions about a seeding experiment depending upon experimental design, methods of analysis and evaluation, and especially on the criteria adopted for success or failure.

In the case of the Tasmania experiment the evidence does not provide strong support for a positive seeding effect. Taking the three operational years together, there is no season in which a  $T_s/T_u^*$  ratio greater than unity is confirmed by comparison with the control-area rainfall through application of the double ratio. Only in autumn of 1968 were both ratios greater than unity (see Table III) but, in view of the small number of operational periods, the result is not significant.

**Table III.** *Tasmania—rainfall analysis for autumns of seeded years*

	<i>n</i>	<i>T</i> <i>inches</i>	<i>C</i> <sub>1</sub> <i>inches</i>	<i>C</i> <sub>2</sub> <i>inches</i>	<i>T/C</i> <sub>1</sub>	<i>T/C</i> <sub>2</sub>	<i>T</i> / $\frac{1}{2}(\text{C}_1 + \text{C}_2)$	<i>T</i> <sub>s</sub> / <i>T</i> <sub>u</sub> *	$\frac{\overline{C}_s}{\overline{C}_u}$
1966	<i>S</i> 4	4.62	3.91	2.80	1.18	1.65	1.38	0.61	1.41
	<i>U</i> 3	5.68	6.60	5.07	0.86	1.12	0.97		
1968	<i>S</i> 4	8.53	9.05	6.81	0.94	1.25	1.08	1.44	1.26
	<i>U</i> 4	5.94	7.05	6.87	0.84	0.86	0.85		
1970	<i>S</i> 5	6.08	4.93	3.72	1.23	1.63	1.41	0.57	1.13
	<i>U</i> 3	6.45	6.10	4.24	1.06	1.52	1.25		

*T*, *C*<sub>1</sub>, *C*<sub>2</sub>, *S*, *U*, *n* as defined in Table I.  $\overline{C} = (\text{C}_1 + \text{C}_2)/2$ .

\* calculated per period.

Moreover, there is no independent evidence that the structure, evolution and constitution of the clouds was so significantly different in autumn as to make them more responsive to seeding.

### 3. Florida Area Cumulus Experiment (FACE) 1970–76

**3.1. Objective.** The object of the experiment was to determine whether seeding with silver iodide pyrotechnic flares from aircraft could be used to augment convective precipitation over a substantial area ( $1.3 \times 10^4 \text{ km}^2$ ) in southern Florida by promoting the growth and amalgamation of supercooled cumulus clouds.

**3.2. Experimental design.** This was a statistically designed experiment without a control area in which seeding of the clouds over the target area was carried out on only half of the 'suitable' days selected at random, the other half being unseeded and used as controls.

Three different types of silver iodide flare were used in the course of the experiment. In the early years a seeding operation involved dropping more than 60 rather low-yield flares into clouds over the target area but, during 1975–76, larger (NEI) flares each producing about 1000 times as many active ice nuclei as the earlier types, were used on the last 17 seeding operations of the experiment.

The response to seeding was judged mainly in terms of comparing the quantity of rain falling in the target area on the seeded (*S*) days with that falling on the unseeded (*U*) days. The total rainfall in the target area was estimated by using a rather old 10 cm radar, the radar estimates being adjusted after comparison with a small network of conventional rain-gauges.

The target area was defined in two different ways and separate analyses made for each. The Floating Target (FT) comprised the radar echoes of all the seeded clouds and those which merged with them so long as they remained in the target area. This may be regarded as the most intensely treated target. The Total Target (TT) comprised *all* the radar echoes including the FT echoes in the fixed target area. The rainfall was measured and analysed over a 6 h period following the first seeding and for the interval between the first seeding and 1 h after the last seeding.

The experimental sample consisted of 39 seeded and 36 unseeded days over the 6-year period but 4 days of disturbed weather ('naturally rainy days') were excluded from the analyses.

3.3. *Working hypothesis.* The main working assumption was that massive injections of silver iodide nuclei would liberate large quantities of latent heat through the induced freezing of supercooled cloud droplets and the deposition of water vapour on to the ice crystals. This latent heat could be expected to increase the natural buoyancy of the clouds, enhance their growth and promote their amalgamation into larger cloud systems which would produce more rain than the individual clouds on their own.

3.4. *Claims.* The authors (Woodley *et al.* 1980) claim that over the period as a whole the ratio of the average rainfall on seeded days to that on unseeded days was about  $1.5 \pm 0.4$  for the TT area, these figures being significant at the 5 per cent level. The differences on seeded and unseeded days are said to be greater and of greater statistical significance on days with light winds, when the cloud radar echoes were mobile, and on days when the more efficient NEI flares were used. The authors admit, however, that the apparent increases in rainfall on the seeded days were due largely to heavy rains on only 4–7 of the seeded days.

3.5. *Assessment of FACE results.* The principal results of the experiment are summarized in Table IV, those for the floating target (FT) and the total target (TT) being listed separately.

Taking all the operational days together, with no stratification, we find that the average rainfall per seeded day exceeded the average per unseeded (control) day by a factor of 1.62 (FT) and 1.27 (TT)

Table IV. Summary of FACE rainfall results

		<i>n</i>	<i>T</i>	Floating Target			<i>T</i>	Total Target		
				$\bar{R} = T/n$	$\bar{R}_s/\bar{R}_u$	$\sigma$		$\bar{R}$	$\bar{R}_s/\bar{R}_u$	$\sigma$
All days	<i>S</i>	38	165.9	4.37	1.62	0.03	246.8	6.49	1.27	0.12
	<i>U</i>	33	88.9	2.69			168.5	5.10		
All days except 7 wettest S days	<i>S</i>	31	93.3	3.01	1.11		154.7	4.99	0.98	
	<i>U</i>	33	88.9	2.69			168.5	5.10		
All days with echo motion	<i>S</i>	28	113.6	4.06	1.75	0.02		6.13	1.57	0.02
	<i>U</i>	17	39.4	2.32				3.90		
Days with no echo motion	<i>S</i>	10	52.3	5.23	1.37	0.14		7.63	1.07	0.37
	<i>U</i>	17	65.1	3.83				7.14		
Days with > 60 low-yield flares	<i>S</i>	22	84.4	3.84	1.08	0.39		5.81	0.89	0.29
	<i>U</i>	19	67.4	3.55				6.54		
Days with NEI high-yield flares	<i>S</i>	17	87.1	5.12	2.06	0.008		7.43	1.72	0.01
	<i>U</i>	18	44.8	2.49				4.32		
Exclude 4 wettest S days	<i>S</i>	13	46.3	3.56	1.43			5.40	1.25	
Days with echo motion and high-yield flares	<i>S</i>	12	62.0	5.16	4.41	0.005		7.34	2.87	0.008
	<i>U</i>	8	9.38	1.17				2.56		
Exclude 4 wettest S days	<i>S</i>	8	21.2	2.65	2.26			4.48	1.75	

*n* = number of days.

*T* = total rainfall ( $\text{m}^3 \times 10^7$ ).

$\bar{R}_s$  = average rainfall per seeded day.

$\bar{R}_u$  = average rainfall per unseeded day.

$\sigma$  = statistical significance level.

but these results are heavily weighted by very heavy rains falling on 7 of the 38 seeded days. If these are excluded, the  $\bar{R}_s/\bar{R}_u$  ratios drop to 1.11 and 0.98 respectively.

The authors claim that clear-cut positive effects of seeding are apparent only on days when the radar echoes from the clouds were mobile but, in fact, though the  $\bar{R}_s/\bar{R}_u$  ratios were greater on those days, the actual average daily rainfalls were greater, both on seeded and unseeded days, when the echoes were stationary.

Although greater positive seeding effects are claimed for the high-yield NEI silver iodide flares, the actual average daily rainfalls (excluding the four wettest days) were considerably *lower* than on the days when low-yield flares were used despite the fact that during the 1976 large-flare campaign the radar rainfall totals were adjusted upwards by 32 per cent for the seeded days and by only 10 per cent for the unseeded days. However, because the rainfall on the unseeded days was also a good deal lower in 1976 than in previous years when the low-yield flares were used, the  $\bar{R}_s/\bar{R}_u$  ratios for the trials with NEI flares come out higher at 1.43 (FT) and 1.25 (TT) even if the four seeded days of heaviest rain are excluded.

The highest  $\bar{R}_s/\bar{R}_u$  ratios were obtained when high-yield flares were used on days of radar echo motion but again the results are heavily weighted by the four days of heavy rain. But, in any case, the high ratios are the result of the rainfall being abnormally low (about half the overall average) on the unseeded (control) days rather than the rainfall on the seeded days being abnormally high.

The authors also claim that rainfall increases attributable to seeding were of greater magnitude and statistical significance on days with light winds ( $\leq 6$  kn), when the clouds (radar echoes) were mobile, and when the high-yield flares were used. In fact, on only two of the seven days of heavy rain, which were largely responsible for the apparent positive effects of seeding, were all three criteria satisfied and, on these, the radar estimates of rainfall were multiplied by factors of 3.0 and 2.2 to adjust them to the rain-gauge data.

It is questionable whether a sample of fewer than 40 pairs of seeded and unseeded (control) days spread over six years is large enough to be stratified into several groups and produce results that are statistically significant. Moreover, we have to note the low accuracy of the rainfall measurements, the large adjustment factors applied to them, the distortion of the results by the fact that 44 per cent of the total rain fell on only seven (18 per cent) of the seeded days, and that when these are excluded there is little difference between the rainfall on the seeded and unseeded days.

The authors' basic physical premise that seeding promotes the growth and amalgamation of the clouds which then produce more rain than the individual clouds would do on their own receives little support from the data. Although the report mentions a few observations (not connected with seeding) of merged radar echoes producing heavier rain than isolated shower echoes, and opines that such mergers require strong surface-wind convergence for at least one hour beforehand, there is no evidence that seeding of isolated cumulus can lead to enhanced low-level mesoscale convergence. As indirect evidence for seeding leading to cloud mergers the authors cite the fact that the ratio of the FT to the TT rainfall on seeded days was greater (0.67) than on unseeded days (0.53) which they associate with a greater tendency for radar echoes to merge with the seeded clouds. However, the figures of Table IV are less favourable to this thesis on echo-motion days than on days of no motion; moreover, there is no difference between occasions when high-yield and low-yield seeding flares were used. The evidence is strongest for the few occasions when high-yield flares were used on days of echo motion but again this is strongly biased by the four wettest seeded days.

It may be that seeding on these four days led to considerable cloud growth and amalgamation but there is no direct evidence for this and if it occurs so rarely it can be of little practical significance.

Again we come to the conclusion that if these few days are excluded from the analysis there is no



convincing evidence of any kind that seeding during the FACE operations produced a significant increase in rainfall.

After this analysis was completed, there appeared an important paper by Nickerson (1979) which comes to similar conclusions. His careful examination of hourly rainfall records reveals that prior to the introduction of the high-yielding NEI flares the average rainfall *outside* the target area was slightly greater on seeded days than on unseeded days. During the period when the NEI flares were used the average rainfall outside the target area on seeded days exceeded that on unseeded days by 60 per cent even in the 3 h period before seeding commenced, whilst during the 6 h period after the first flare was released,  $\bar{R}_s/\bar{R}_u$  ratios in the outside (control) area as high as 3 were recorded. Nickerson also found that the large  $\bar{R}_s/\bar{R}_u$  ratios that occurred over the target area during the NEI-flare period were *not* associated with anomalously high rainfall on seeded days but rather with abnormally low rainfall on unseeded days. Indeed when he used the control area data to compute a double ratio (as described for the Tasmania experiment) he obtained a value of only 0.82.

Nickerson accordingly concludes that the statistically significant differences between area-wide rainfall on seeded and unseeded days during FACE was due to natural variability rather than to seeding and also draws attention to the dominating effect on the statistics of a few 'outlier' days of heavy rainfall.

#### 4. The Israeli experiment (1969–75)

4.1. *Objective.* The objective of the Israeli experiment as described by Gagin and Neumann (1980) was to examine the possibilities of enhancing rainfall in the catchment area of Lake Kinneret (Tiberias or Sea of Galilee) in Israel by seeding winter continental cumuliform clouds with silver iodide smoke released from aircraft supplemented by ground generators, and to determine the optimum cloud conditions for achieving a positive result.

4.2. *Experimental plan.* The experiment was conducted during the six winter seasons of 1969–75 following the encouraging results obtained in a preliminary experiment carried out from 1961–67 and described by Gabriel (1970), Gagin and Neumann (1974).

The total target area of 3775 km<sup>2</sup>, subdivided into eight sub-areas, covering much of northern Israel including most of the Lake Kinneret catchment, has a mean annual rainfall of 630 mm, about two-thirds falling in the winter months. This rainfall was measured by about 55 rain-gauges read daily. A control area lying to the west and upwind of the target area, between it and the Mediterranean coastline, contained 25 rain-gauges, the correlation coefficient between the historical values of the rainfalls in the complete target and control areas being about 0.9 whilst that between the Lake Kinneret Catchment (LKC) and the control was 0.85.

The operational period was divided into seeded and unseeded days allocated at random but these counted as experimental days only if more than 0.1 mm of rain had fallen at any one of three specified stations in an unseeded buffer zone to the south of the target area. Each winter operation consisted of about 60 experimental days, about half of these being seeded. The clouds were mostly clusters or bands of cold cumuliform clouds associated with cold fronts and post-frontal systems moving in from the Mediterranean. The cloud-base temperature was typically  $-5$  to  $-8$  °C with tops extending frequently to the  $-15$  to  $-20$  °C levels. Aircraft seeding was carried out at cloud-base level along a north–south line located some 35 km west of the centre of the target area and was supplemented in the extreme eastern part of the target by ground generators located on the windward sides of hills.

The effects of seeding were judged in much the same manner as for the Tasmania experiment by comparing the average daily rainfalls in the target and control areas on both seeded and unseeded

days selected at random in terms of the double ratio  $(T_s/C_s)/(T_u/C_u)$ , its standard error and its statistical level of significance.

4.3. *Assessment of the results.* Table V shows that the mean daily precipitation on the days randomly allocated for seeding exceeded that on the days designated to be unseeded by 16 per cent over the target area as a whole and by 21 per cent in the more limited Lake Kinneret catchment. Rainfall in both areas significantly exceeded the longer-term (1949–60) average values on both seeded and unseeded days. In the control area the average daily rainfall was only 3 per cent higher on seeded than on unseeded days so that increases, on computed double ratios, of 13 per cent and 18 per cent were ascribed to the effects of seeding in the total and LKC target areas respectively. The excess of the double ratios over unity were at least twice the standard error and both results are statistically significant at better than the 3 per cent level. These results are in rather good agreement with those obtained in the preliminary (1961–67) experiment which indicated rainfall increases of 15 per cent overall due to seeding.

Furthermore the eight sub-target areas all received more rain on the seeded than on the unseeded days as indicated by double ratios ranging from  $1.06 \pm 0.04$  to  $1.27 \pm 0.10$ . The latter high value, significant at better than the 1 per cent level, was for the central part of the target area.

Table V. *Israeli experiment (1969–75)—rainfall analysis*

		Mean daily precipitation millimetres	$T_s/T_u$	$DR = \frac{T_s/C_s}{T_u/C_u}$	$\sigma$
Total target area	<i>S</i>	$8.81 \pm 0.74$	1.16	$1.13 \pm 0.06$	0.028
	<i>U</i>	$7.59 \pm 0.68$			
	<i>H</i>	$6.81 \pm 0.36$			
LKC area	<i>S</i>	$8.89 \pm 0.76$	1.21	$1.18 \pm 0.08$	0.017
	<i>U</i>	$7.32 \pm 0.68$			
	<i>H</i>	$6.33 \pm 0.37$			
Control area	<i>S</i>	$8.30 \pm 0.68$	$(C_s/C_u)$ 1.03		
	<i>U</i>	$8.05 \pm 0.70$			
	<i>H</i>	$7.64 \pm 0.37$			

*H* = long-term average (1949–60).

DR = double ratio.

LKC = Lake Kinneret catchment.

Other symbols as in Table I.

These statistical results provide much more convincing evidence for the positive effects of seeding than those for either the Tasmania or FACE experiments although the basic Israeli data have not yet been published in sufficient detail to facilitate a full independent assessment. It is, however, unlikely that the results and conclusions are strongly biased by the occurrence of abnormally heavy rains on only a few seeded days as in FACE because, as Table VI shows, the largest percentage increases in rainfall attributed to seeding were obtained on days of light or moderate rainfall with rather little contribution from days when the rainfall exceeded 15 mm.

Further support based on physical arguments comes from an analysis of the rainfall data stratified according to the modal value of the daily distribution of cloud-top temperatures as shown in Table VII. When the modal value of the cloud-summit temperatures lies within the range  $-15$  to  $-21$  °C the indicated increase of rainfall due to seeding is 46 per cent, significant at the 0.5 per cent level. On other days, when the cloud-top temperatures are either above  $-10$  °C or below  $-21$  °C, the seeding effects are either nil or statistically insignificant. Gagin and Neumann give this result the following plausible (but over-simplified) physical interpretation. In clouds warmer than  $-5$  °C the growth of ice crystals nucleated by seeding is so slow that they have neither the time nor space in which to grow into rimed

**Table VI.** *Israeli experiment—correlation of seeding effect as measured by double ratio with precipitation amount in control area*

Control precipitation amount (millimetres)	Number of days	Double ratio	$\sigma$
All the data	388	$1.13 \pm 0.06$	0.028
0–30	367	$1.12 \pm 0.06$	0.047
0–25	358	$1.13 \pm 0.07$	0.038
0–20	345	$1.14 \pm 0.07$	0.031
0–15	319	$1.23 \pm 0.08$	0.005
0–10	275	$1.17 \pm 0.09$	0.047
0–8	241	$1.24 \pm 0.12$	0.034
0–6	217	$1.20 \pm 0.14$	0.079
0–3	158	$1.32 \pm 0.16$	0.053
0–1	94	$1.67 \pm 0.27$	0.030
0–0.5	62	$1.96 \pm 0.38$	0.056

**Table VII.** *Israeli experiment—correlation of seeding effect as measured by double ratio with cloud-top temperature*

Cloud-top temperature (modal value in degrees Celsius)	Number of days	$DR = \frac{T_s}{T_u} / \frac{C_s}{C_u}$	$\sigma$
$-15 \leq T$	85	$1.22 \pm 0.13$	0.087
$-26 \leq T < -11$	112	$1.19 \pm 0.09$	0.017
$-26 \leq T < -15$	77	$1.24 \pm 0.11$	0.017
$-21 \leq T < -11$	77	$1.29 \pm 0.11$	0.008
$-21 \leq T < -15$	42	$1.46 \pm 0.16$	0.005
$T < -26$	87	$0.94 \pm 0.11$	0.629
$T < -21$	122	$0.96 \pm 0.09$	0.492
$-26 \leq T < -21$	35	$1.01 \pm 0.15$	0.503
$-11 \leq T$	50	$1.31 \pm 0.20$	0.602

precipitation elements before either the cloud dies or they are carried up into the tops of the clouds reaching much colder levels. In any case, the silver iodide generators produce only a very low yield of nuclei active at  $-5^\circ\text{C}$ .

In clouds with summit temperatures below  $-21^\circ\text{C}$  the concentration of natural ice nuclei is probably sufficient to initiate the growth of solid precipitation elements in such numbers and sizes that they consume the cloud liquid water as fast as it is condensed in the updraught. In this case, the supply of additional artificial nuclei is unlikely to have much effect and this may explain why seeding did not apparently augment the heavy rains which presumably fell from deep clouds.

It is at intermediate temperatures that seeding is liable to have greatest effect since the ice crystals, growing at the maximum rate by diffusion at about  $-15^\circ\text{C}$ , are most likely to grow into precipitation elements before the cloud dies or before they are carried up to higher levels where they would have to compete with increasing concentrations of natural crystals.

Apparently the Israeli clouds do not contain the high concentrations of natural ice crystals that are frequently observed in only slightly supercooled clouds of maritime origin and cannot sustain an ice-crystal multiplication process such as that described by Hallet and Mossop (1974). This may be a consequence of Gagin's (1975) observation that these clouds consist of high concentrations of small droplets with a narrow range of sizes but this is unlikely to be the only reason. It is therefore important to determine any microphysical and/or dynamical differences that may account for these clouds being more responsive to seeding than those in many other parts of the world, if only to guide the choice of location and design for other experiments.

## 5. Conclusions

Examination of the results of three recent major long-term cloud-seeding experiments carried out in widely separated parts of the world reveals how different conclusions can be drawn depending upon the experimental design, methods of analysis and evaluation and on the criteria adopted for success or failure. In the case of the Tasmania and Florida experiments the evidence does not provide strong support for a positive seeding effect. Statistical evaluation of the Israeli experiment provides much more convincing evidence for average increases of rainfall due to seeding of about 15 per cent, but why these convective clouds should be more responsive to seeding than rather similar clouds in other parts of the world is not clear. It is therefore important to make comparative studies of the microphysical and dynamical evolution of these and other apparently similar cloud populations in an attempt to resolve these questions.

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551.506:581.3

## The Meteorological Office archive of machinable data

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### Summary

This paper describes the general layout of the contents of the Meteorological Office archive of machinable data. The principles adopted to quality control the data are discussed together with methods of storage, cataloguing and use. Some indication is given of the future development of the archive.

### Introduction

The Meteorological Office has maintained a collection of machinable data for more than 20 years, but an organized archive of data to be retained permanently has emerged only during the last 10 years. The purchase of an IBM 360/195 computer early in the last decade acted as a catalyst because it provided improved peripheral devices such as magnetic discs as well as increased processing power.

General purpose access routines for the proposed archive were developed by the Systems Development Branch of the Meteorological Office and written in IBM assembler code. This approach demanded that certain key parameters were located in particular positions in all data sets in the archive. This minor disadvantage was outweighed by the power of the access software, which included a number of index search and match routines, and the ability of any programmer, who had been trained to use the access method, to access any data set designed to this specification.

The first data sets which were created contained climatological data. This was a logical choice because many of the data already held on magnetic tape were in this class, and pressure generated by enquiries and investigations indicated that they should be considered first. A little later, synoptic data were also archived permanently in a data set complying with the general guidelines. However, these data were not accessed frequently because the format did not match that used by the Central Forecasting and Forecasting Research Branches. Demand for these data is now increasing, but since they are not required for forecasting, storage in synoptic order has proved to be inconvenient and the data are now being re-sorted and archived in a more suitable order.

The priority allocated to the development of any section of the archive was dictated by the operational requirement for the data rather than by a rigid pre-arranged schedule. A manuscript catalogue of machinable data was produced listing all data sets in the machinable archive plus all collections of data which have been declared by individual branches. There was limited effort available for cataloguing because priority was given to producing master lists of observing stations which were more urgently required.

The number of data sets, both inside the archive and maintained parochially by individual Meteorological Office Branches, has increased to the point where lack of adequate cataloguing has become an embarrassment. It has also become evident that some data sets were ill-suited for their intended roles and others no longer necessary. A rationalization exercise has been carried out and the results are now being implemented. The remainder of this paper will attempt to describe the planned final state of the machinable archive, and to indicate what has already been achieved.

### **A general description of the archive of machinable data**

The archive of machinable data can be conveniently divided into a number of logical sections as shown in Figure 1. DATAMAST, the proposed on-line catalogue of machinable data is shown as the top level of the archive since it would be the logical starting point for a user. Apart from the various master indexes, data are stored in four different orders as follows:

- (a) *Synoptic order.* All data for a particular hour are stored together. Within the hour, the data are organized in station or location order.
- (b) *Periodic order.* All data for one station or location for a nominated period are stored together, followed by the next station for the same period. Each station record is stored chronologically.
- (c) *Climatological order.* All data for a particular station or location for all times are stored chronologically.
- (d) *Regional order.* All data from a nominated geographical region are stored in any of orders (a) to (c) above.

The individual data sets do not always stand alone; there is sometimes a flow of data between them. Such data flows are indicated in context in the following descriptions of each of the elements of the archive.

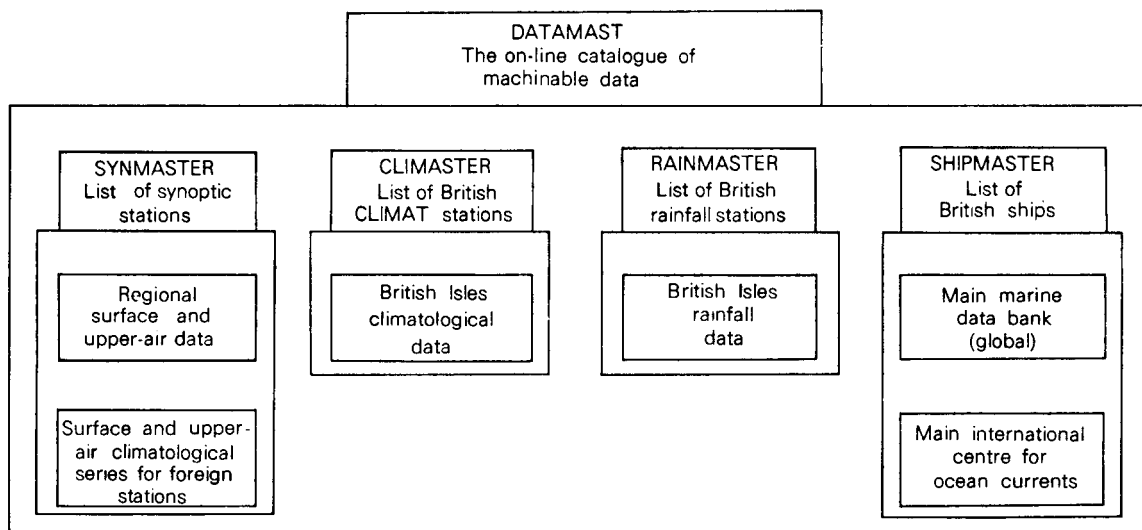


Figure 1. A schematic representation of the archive of machinable data.

### 1. DATAMAST—an automated catalogue of machinable data

The manuscript catalogue of machinable data is produced manually and updated at the end of each year. Entries are often out of date, and very few details are included because manual amendment is so laborious. The present catalogue is not effective, much staff time being wasted in searching for the appropriate data to answer enquiries. There is also evidence that the best answer is not always given because the person concerned is not aware of the existence of specific data sets. As the number of data sets increases this problem will grow and the difficulties experienced in updating the present catalogue will multiply.

As part of a major review to determine the number and range of data sets held in Meteorological Office Branches, it was found necessary to produce a preliminary version of the catalogue of machinable data as an on-line data set (DATAMAST) which could be accessed via a visual display unit (VDU) using the Time Sharing Option (TSO). If it is decided to release a complete operational version of the data set for general use, only minor improvements will be required to the software to make it a little more user orientated. The access routines would display a series of 'menus' to the operator, together with comprehensive instructions, allowing him to specify various parameters such as area, data type, frequency of observation, etc. The access routines would be flexible, and would allow for the whole range of searches from the very general to the specific. Finally the operator would be presented with a short list of data sets enabling him to inspect the attributes of each in turn. The level of information in DATAMAST would depend on the originating Meteorological Office Branch; for instance some branches might wish to withhold volume serial numbers for parochial data sets. A loose-leaf-format volume would also be produced and issued to Branches. This volume would be referred to by entries in DATAMAST and would eventually give the formats of all data sets in the catalogue.

Initially DATAMAST would refer only to machinable data, but eventually it might be extended to manuscript data and linked to the library index system. A considerable amount of work has been done on such data dictionaries in the United States of America, where the philosophy has been developed

that all data should be catalogued in one place regardless of the storage medium or location. This is essentially a practical approach which admits that all data can never be converted into machinable form but the whereabouts of non-machinable data is important.

## **2. Synoptic Data**

(a) *Surface*. Once a day, surface synoptic data are extracted from the synoptic data bank and archived for permanent retention. The end product is a series of half-monthly synoptic data sets containing all synoptic information available from the Global Telecommunication System, and the British Isles and International data sets which contain extremes of temperature, sunshine, six-hourly rainfall and state of ground for the British Isles and north-west Europe respectively. A master list of station details called SYNMASTER controls the selection of stations which are archived. This master list should be checked annually against a reference list on magnetic tape, which is produced by the World Meteorological Organization. Software is being written which will incorporate any amendments which are necessary.

The synoptic extraction program is being rewritten to make it more robust and also to extract climatological information which will be available when the new synoptic codes are introduced in January 1982. The British Isles and International data sets could be discontinued from the inception of the new codes but existing enquiry software is based on these data sets and it is more cost-effective to continue to form them.

Very few enquiries are received which require data in synoptic order and therefore the synoptic data sets are to be reformatted as shown in Figure 2. Only the regional monthly periodic data set and a climatological archive for selected overseas stations will be retained permanently. The climatological archive will be welcome for enquiry purposes although it will not contain all the parameters normally expected and the resolution of the data will not be as good as that derived from climate returns until the synoptic code changes in 1982.

When the new codes are available, monthly climatological data sets will be formed for British stations in the format currently used. At the same time a manuscript return will be produced which can be sent to the reporting station for quality control. Capturing climatological data from the synoptic data bank in this way will ease the work load of the data keying section.

(b) *Upper air*. An analogous system to that used for surface data is being developed for upper-air data. Once again synoptic ordering has proved inappropriate and regional monthly periodic data sets are to be formed for permanent retention. These data sets will be used to create a climatological series for selected overseas stations which will provide data to answer enquiries. The complete system is shown schematically in Figure 3.

## **3. British Isles climatological data**

(a) *Surface*. Climatological data from official Meteorological Office stations will be captured from the synoptic archive as described above. Data from co-operating stations which are not connected to the telecommunication system do not appear in the synoptic data bank and will therefore continue to be keyed to magnetic disc or will be collected in some other way. The various options are discussed in detail in a later section on data capture. As shown in Figure 2 an increasing amount of data will be received on magnetic cassettes from field loggers such as DALE described by Burtonshaw and Munro (1977), the automated Sferics system, Lee (1980) and the proposed Meteorological Office Automatic Climatological Recording Equipment (ACRE).

(b) *Upper air*. All the main radiosonde stations in the United Kingdom are now equipped with the Mk 3 radiosonde system. The system copies a selection of data from each ascent on to a magnetic

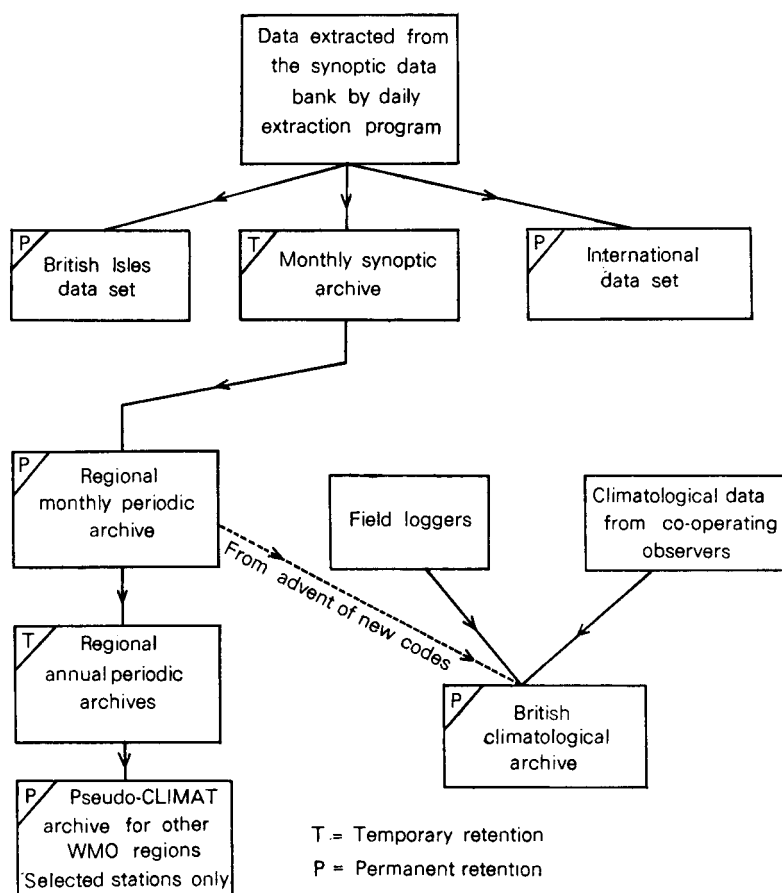


Figure 2. The surface data system.

cartridge cassette which is sent to Bracknell twice a month for processing. Since the necessary data for the climatological archives are retrieved from the cassette, manuscript returns are no longer completed except in cases of equipment failure. The standard-level data are extracted and added to the climatological upper-air archive; the special points which give fine structure are archived separately in the new detailed climatological archive. The main climatological archive is completed by retrieving data from the synoptic data bank as shown in Figure 3 when data are not available from the cassettes.

Balthum data are also received in manuscript form, keyed to disc and archived in a climatological series. The same data set has provision for MAST data at several levels.

#### 4. The rainfall data system

RAINMASTER contains a wealth of information about rainfall stations and is probably the most frequently used of the master lists. It is now the practice to omit all station details except the station number from data sets to avoid the problems of multiple corrections; one correction to RAINMASTER suffices.



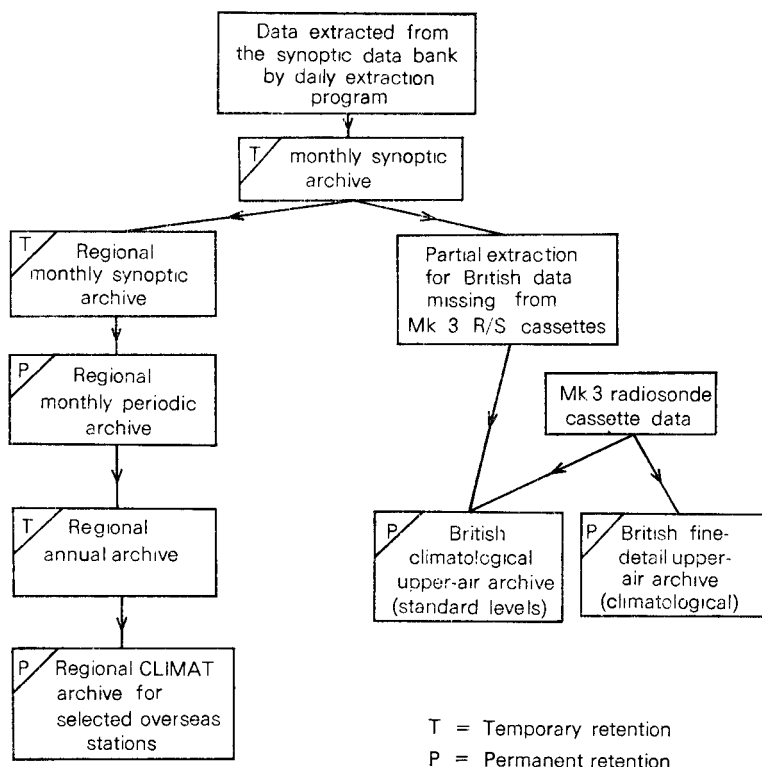


Figure 3. The upper-air data system.

Individual rainfall data sets are created for each different frequency of observation using either periodic or climatological order. Data from devices which measure rainfall with a fine time-resolution are assembled in one data set with markers which indicate the device or method of data extraction.

Data derived from the radar network are an exception and will be kept separately regardless of the frequency of observation. However, daily rainfall totals measured by radar will be combined with rain-gauge values to produce a data set containing daily areal rainfall totals. Storage facilities could not possibly cope with the enormous amount of data generated if rain-gauge data, radar-derived grid-point rainfall data, and the composite fields were all archived. Software will be developed to create composite fields in answer to individual enquiries as they occur.

The main data sets in the rainfall archive are shown in Figure 4. Daily rainfall totals are transferred from the climatological archive to the daily rainfall archive at regular intervals.

## 5. The marine data system

The marine data system does not yet have a master list in machinable form but the necessary information to create one is available on manuscript. The main marine data bank and its satellites, the ocean weather ship and the light-vessel data sets, are climatologically ordered. Data from the mobile ships are not put straight into the main marine bank; they are first stored in the annual periodic data set regardless

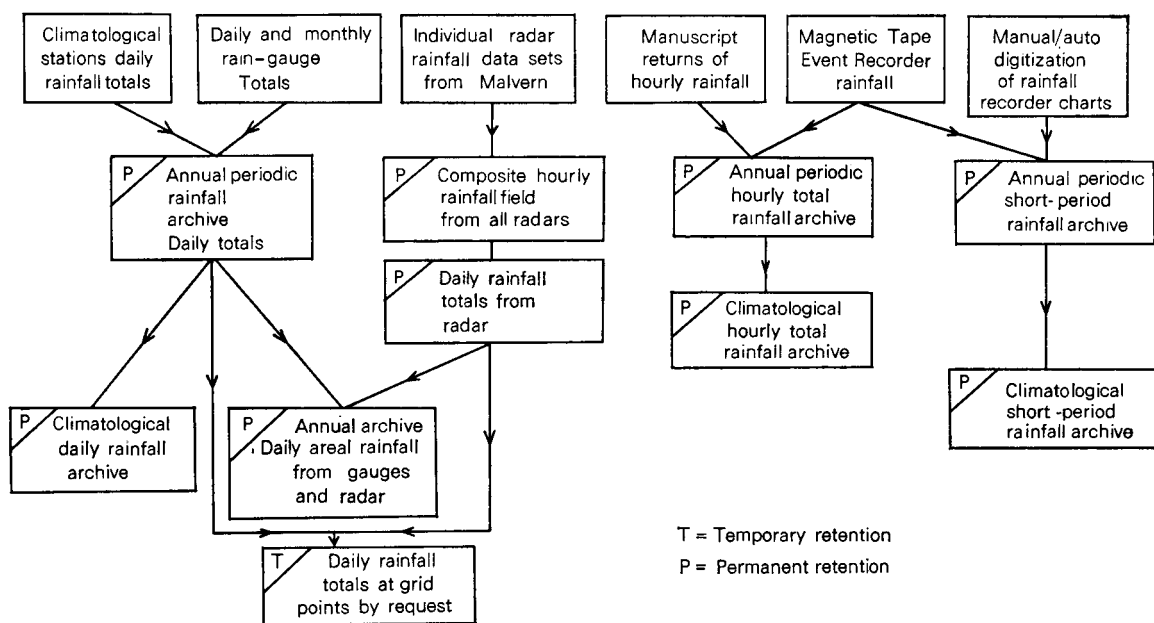


Figure 4. The rainfall data system.

of whether they have originated from the fleets of foreign countries or our own merchant ships. At this stage quality control is done and data recorded by our ships while in other regions of responsibility are reformed and dispatched to the appropriate centre in the International Exchange Format.

Many marine enquiries are received before data become available to answer them because there is a considerable delay between making the observation at sea and receipt of the ship's meteorological log-book at Bracknell. This problem is partially solved by creating an annual data set using data retrieved from the synoptic data bank as shown in Figure 5. Less than 50 per cent of the data eventually received can be captured in this way but it is useful to have even this limited information until manuscript returns are processed. When data from the manuscript returns exceed those from the synoptic data bank, the temporary data set is deleted.

The Meteorological Office is also the international centre for archiving ocean current data measured by mobile ships. A climatological data set has been created with a similar format to existing marine data sets and software is being developed to reformat the data for international exchange, as required.

## 6. Quality control

It is beyond the scope of this paper to consider in detail the various quality control methods used on archive data but the general principles can be summarized. There are usually several levels in any quality control system starting with the simplest checks and becoming progressively more complex, and most systems incorporate human scrutiny at some stage. It is a general rule that data must never be destroyed by quality control action and the archive of machinable data obeys that rule. Separate cross-referenced data sets contain suspect data together with the reasons for the change; the original

Met.O.931

THE  
METEOROLOGICAL  
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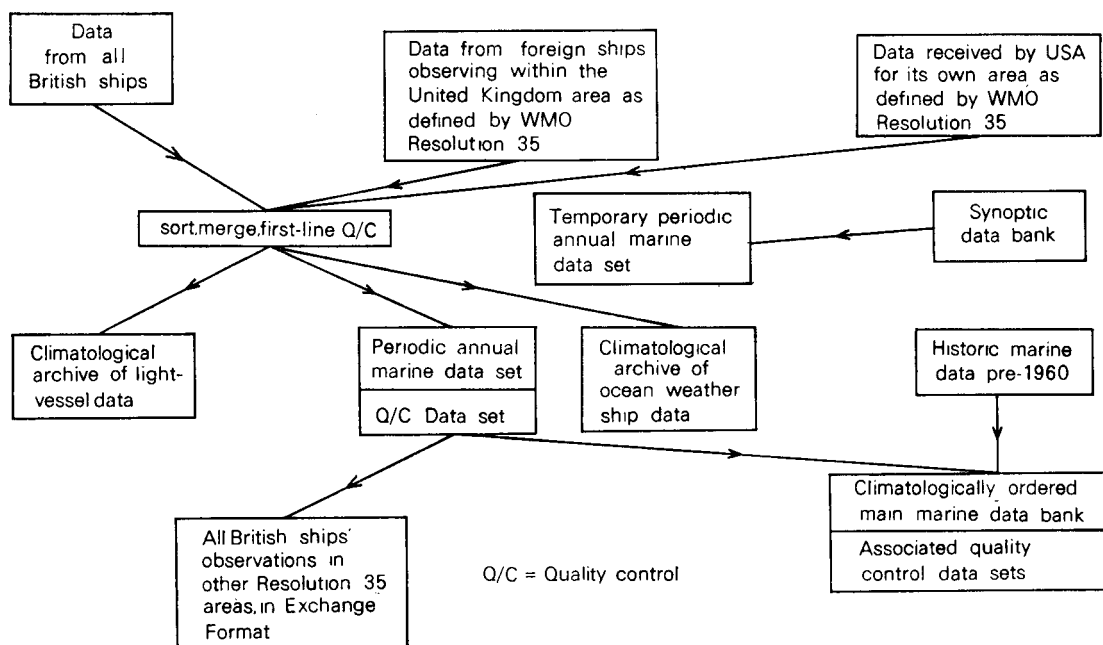


Figure 5. The marine data system.

data set will contain either missing data or, if possible, an estimated value, with a marker set in both cases. Software exists to restore the original data and display them together with the markers and estimated values on demand.

The levels of quality control applied to data in the machinable archive vary from virtually negligible for surface synoptic data through an average level for marine data to highly detailed for daily rainfall and climatological data. Table I summarizes the situation and indicates where improvements are in hand. The level of quality control is usually determined by the use to which the data will be put and the cost of processing them. Many of the climatologically ordered data sets contain data reformed from periodic data sets which are used to answer enquiries for data at a given location and time. Such data must be well quality controlled, therefore the climatological data receive a higher level of quality control than usual, of necessity. Much of the quality control is aimed at correction of random errors which occur after observation, but recently an attempt has been made to present summarized quality control information to rainfall station inspectors via a VDU. The inspector is then able to consider the performance of the rainfall station in the context of the data produced. A similar system has been requested for other climatological stations and is the first recognition of the fact that the quality of the machinable archive is dependent on the whole system which is used to produce it. The system of production may only be improved by encouraging feedback of information at all stages, including the observers.

## 7. Capture of data

The majority of climatological data, even from official Meteorological Office stations, is extracted from manuscript returns and keyed direct to disc by the data keying section. The whole operation is dependent on the staffing and workload of this part of the Meteorological Office. Some pressure of

**Table I.** *Quality control in the archive of machinable data*

Data type	Frequency	Quality control method	Status
Synoptic surface	Hourly	As in synoptic data bank	Results not included in permanent archive
Synoptic upper-air	6-hourly	As in synoptic data bank	Operational
United Kingdom climatological upper air	6-hourly	Comparison of standard and selected levels, hydrostatic equation, lapse rates, wind shears, comparison with forecast model input fields	Under development
United Kingdom fine-detail upper-air	6-hourly	Nil	Nil
United Kingdom climatological surface	Hourly to daily	Internal consistency, range checks, area means, comparison with neighbouring stations	Operational
Daily rainfall	Daily	Comparison with neighbouring stations	Operational
Short-period rainfall	Hourly to daily	Nil	Nil
Marine	Hourly to daily	Internal consistency and range checks	About to become operational

work has been removed by automating observing instruments such as the Mk 3 radiosonde system. However, it is likely that many other automatic observing systems will be used to fill gaps in the observing network rather than to replace existing stations.

The acquisition of climatological data from official stations via the synoptic data bank should be effective in reducing the amount of data to be keyed by approximately 20 per cent when the new codes are implemented in 1982. Even so, further reductions are desirable if keying staff are to cope with all the data which are awaiting keying. There are a number of alternatives which could be used to process data from co-operating observers both on land and at sea.

It is possible that an Optical Character Recognition (OCR) system with associated keying stations could be used. These mixed systems allow documents to be read by the OCR and unresolved characters from an earlier batch to be keyed simultaneously. The main problem with such a system is the unpredictable error rate; Meteorological Office forms are larger and more complex than those used in most other applications and the text is often less distinct which may result in higher error rates than those obtained commercially.

An alternative approach is to purchase a number of individual key-to-cassette systems and use these to record observations. The cassettes could be sent to Bracknell to be translated at monthly intervals, or at the ends of voyages for ships. Most systems currently marketed are used for stock control in large warehouses, or for remote ordering of goods using acoustic couplers and Post Office telephone lines. Such systems are too elaborate and much too expensive for the Meteorological Office.

There are indications that some manufacturers could use standard components to produce a compact keyboard and cassette tape drive for a fairly reasonable price (in the region of £200). It could then be a practical solution to issue these units to co-operating observers as inventory items.

## 8. Storage media

The Meteorological Office machinable archive is stored on magnetic tape, each tape having two back-up copies. There are about 4000 archive tapes and a further 11 000 tapes containing individual Branch data sets of one type or another. These tapes occupy valuable storage space and also make



demands on staff and resources since they must be cleaned and respooled at regular intervals. Pressure on storage space has been eased by changing to higher density tapes and ensuring that each tape is full. Unfortunately, this means that some users have to read a greater length of magnetic tape to find the sections which they require and therefore are much more likely to encounter a tape error. This can be offset to some extent by reducing safety margins and allowing users to access the first back-up tape which is copied in reverse order.

Since the archive will continue to grow, a long-term solution must be sought to the problem of permanent retention of machinable data when any part of those data may be required at any time. Mass storage devices could be a practicable solution, but all of those currently available use magnetic tape technology and this is not acceptable for permanent retention of data. Research is being done on systems using laser holography on photographic film, and laser light on optical discs. The former is a sequential device with very long access times for a 1000-metre spool of film, although that film may contain the equivalent of 540 standard (1600 bpi) tapes. The latter is a direct-access device but has a lower capacity equivalent to about 20 magnetic tapes. Both devices have the disadvantage that data may not be amended; the entire film or disc must be rewritten. However, it is possible that this problem may be overcome by the use of laser light of differing wavelengths and photochromic materials whose response is wavelength dependent. Such devices could then be used for data of medium age (two to four years), where there is still the possibility of amendment, and also for updating long records.

## **9. Using the archive of machinable data to answer enquiries**

The Meteorological Office archive of machinable data exists primarily to answer enquiries which vary in complexity from a simple request for data to a large-scale investigation taking many months and involving substantial resources. In general, the larger the task the less impact the storage media will have on the time taken to complete it. Many enquiries are received for small amounts of data, a substantial proportion by telephone. Even if the data are held on a permanently mounted disc, it is not possible to answer such an enquiry immediately because TSO is not designed for the purpose and response is too slow. A microfilm archive is now being developed to satisfy this need, containing data extracted from the machinable archive and printed on microfiche and jacketed 16 mm film. There is little co-ordination of the development, which is occurring simultaneously in most of the enquiry bureaux, and access methods are fairly crude and manually operated. However, these embryo systems have proved successful in answering simple data enquiries both by telephone and with confirmatory hard copy.

A study of available equipment has been made, for systems ranging from simple manually operated readers to fully automatic carousels accessed by minicomputer, such as the ARMS system (Hannum 1979). It is difficult to say what level of automation is justifiable because an expensive system such as ARMS would not be cost effective with the number of small enquiry bureaux which we have at present. Only, if a centralized enquiry bureau was set up, similar to that of the United States National Climatological Center at Asheville, North Carolina, which uses a mixed microfilm and computer data base system, could the capital outlay be justified. However, such a development is not thought to meet Meteorological Office circumstances, even though the addition of word processing facilities and automatic invoicing to such a system would further increase its effectiveness.

## **Conclusion**

This paper has attempted to describe the growth of the Meteorological Office archive of machinable data and to indicate its future development. General purpose access software has been developed for the archive, and higher level accessing subroutines are now being developed for some of the component data sets so that application programmers need not be aware of the mechanics of the indexing used.

This activity together with the cataloguing of available data is the first step towards an automated data base perhaps using new mass storage technology. It is unlikely that the machinable archive alone will ever satisfy all the data enquiries which are received. It will probably be necessary to set up a hybrid machinable/microfilm data base and a true data dictionary to achieve a high success rate.

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## Plotted synoptic charts and time-series graphs for research purposes

By K. Grant

(Meteorological Office, Bracknell)

### Summary

This note describes a facility for the provision of a wide variety of plotted but unanalysed synoptic charts and graphs for use by investigators and research workers.

### 1. Charts

Data for meteorological research and investigation are normally presented as computer printouts, supplemented by either analysed operational charts or simplified versions of them which have been published. The researcher will usually want to reanalyse these charts, and is often prevented from doing so easily by the incompleteness of the data, by the presence of the existing, often simplified, analysis, or by the need to trace the chart to produce his own copy. A suite of programs has now been written which will give the investigator a plotted but unanalysed chart which he can analyse directly, and on which additional observations can be plotted by hand. The orography is not shown. A list of positions and heights can be provided for British Isles synoptic and UK climatological stations.

Data from the synoptic data bank on magnetic tape, which is retained for five years, are normally used to plot the chart on 35 mm microfilm. At present this can be enlarged at Bracknell either to A4 size or, preferably, to chart size (60 cm × 45 cm). The surface charts available are as follows:

(a) A standard Central Forecasting Office surface chart, such as the 1 in 2 million scale British Isles chart produced for facsimile transmission, the 1:3 million British Isles and North Sea chart, the 1:7.5 million western Europe chart or the two 1:10 million charts covering the northern North Atlantic Ocean. There are standard lists of stations for all these charts.

(b) A special 1:2.1 million British Isles chart with an augmented station list giving a rather more detailed coverage than the 1:2 million chart, and with ships' reports plotted also. A printout of the coded observations for stations which cannot be plotted because of lack of space can also be provided with this chart.

(c) A 09 GMT synoptic-type chart of the British Isles using data from climatological stations (see *Meteorol Mag*, **109**, 260) which gives a once-a-day mesoscale depiction of wind, total cloud cover,

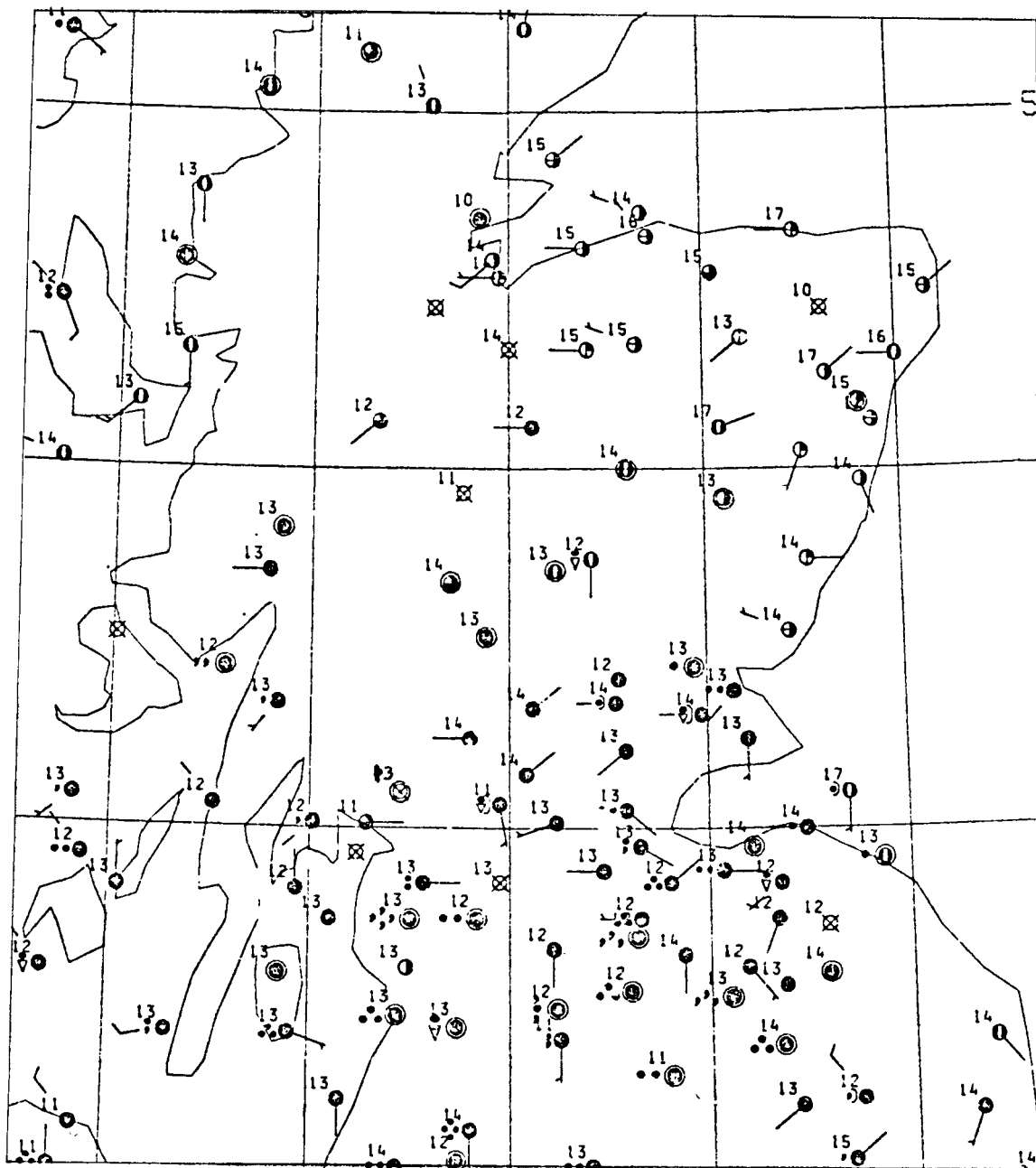


Figure 1. Detail of 09 GMT climatological stations chart for 19 July 1980, showing part of Scotland. Total cloud amount, wind, present weather and temperature are plotted.

present weather, temperature and dew-point, or a subset of these (Figure 1). In contrast to synoptic data bank data, the climatological data used are available for 1972 onwards. For such data, 1:1.5 million charts covering either England and Wales or Scotland and Northern Ireland are also available.

(d) A single-element British Isles chart of any of the daily or 09 GMT meteorological elements recorded by climatological stations, e.g. maximum temperature or 09 GMT temperature (units 0.1 °C), sunshine, snow depth, day of hail, 30 cm soil temperature.

(e) A surface chart centred on any given latitude and longitude in the northern hemisphere and for any scale in whole millions, e.g. 1:3 million, 1:11 million. There is a standard coastline, which is rather crudely drawn at the 1:1 million and 1:2 million scales, and a standard station list which is probably adequate in Europe, but which would not support detailed charts in other parts of the hemisphere. However, all ships' reports whose plots do not overlap are plotted.

Upper-air charts can also be produced for any of the standard levels or for 1000–500 mb or 1000–850 mb thicknesses and thermal winds. The main charts available are the 1:7.5 million western Europe (about 20°W to 20°E), the 1:10 million Europe (rotated, with vertical longitude 35°W) and the 1:20 million circumpolar North Atlantic chart covering Europe, the Arctic, Canada and the North Atlantic Ocean to about 35°N. Reports from aircraft can be plotted, but for 00 GMT they cover the whole period 00–12 GMT (for 12 GMT, 12–24 GMT), thus biasing these reports towards a later time than the nominal one. Satellite data can also be plotted if required.

There are several plotting options available, mainly dependent on which elements are required. The digit size can be altered; increasing it can result in the overlapping of plots, but more usually in the omission of some of the stations to avoid overlapping. On the 1:7.5 million upper-air chart, however, the digit size can be made large enough to be legible on A4 enlargements without affecting the coverage.

## 2. Graphs

A program has been developed to plot a month's climatological data for one station on a single frame of microfilm, in the form of a multiple time-series graph of weather elements observed hourly (or three-hourly). The graph consists of four rows containing eight days each, leaving 'day 32' for annotation. On enlargement to chart size, one hour occupies 0.1 inch horizontally, while 4 inches are available vertically for all the elements. (One inch = 2.54 cm; inches are still the basic unit of length in the plotting system.) One day's data extracted from the graph are shown in Figure 2(a) at approximately the real size. Most elements reported hourly are shown, either as a line graph or symbols or both, or as the coded values. Cloud base and visibility have a logarithmic scale, while hourly rainfall is presented as a histogram. Sunrise and sunset lines are drawn for each day.

A simplified version of the graph which may be suitable for the general public is shown in Figure 2(b). Only temperature, rainfall, relative humidity, wind speed, sunshine (this only for 1980 onwards) and a limited number of single or double letter codes for present weather and wind direction are plotted.

One version of the graph enables hourly climatological data for up to 31 stations for one day (or 15 stations for two days etc.) to be plotted on a single frame. This arrangement may be useful for case studies, when the stations can be arranged to be roughly in their geographical positions. Such graphs can be drawn for data back to about 1970, though the single-station monthly graph is available for a few stations as far back as 1949. Case-study graphs from the synoptic data bank can be produced, but are more expensive in computer time than the others, although they can be obtained immediately after the event, while a wait of two to six weeks may be necessary to obtain quality controlled climatological data.

Modifications to the formats of the graphs could be made if experience of their use showed a need for change.

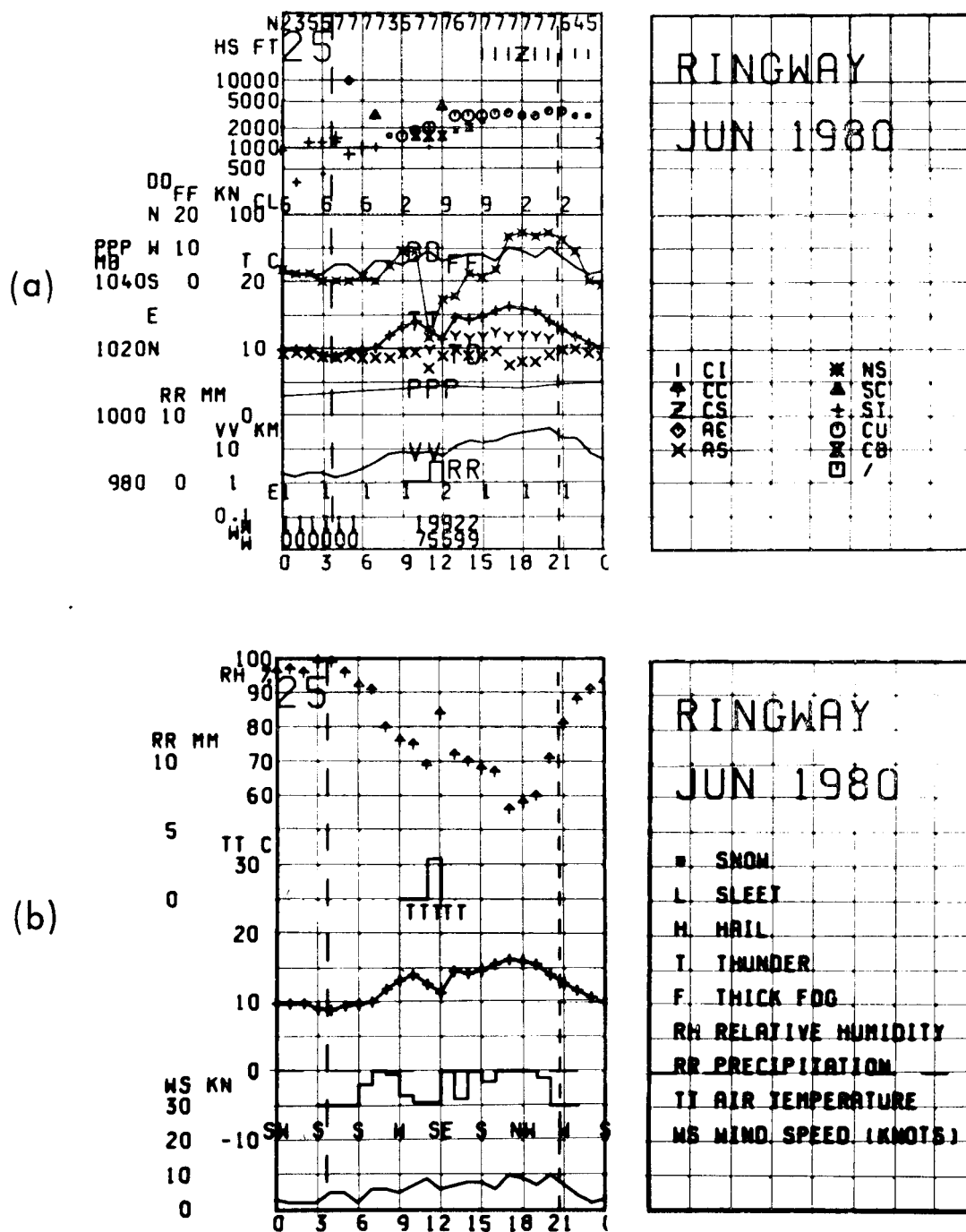


Figure 2. Time-series graphs of one day's weather, extracted from monthly graphs. (a) Standard version. (b) Simplified version.

### 3. Costs and enquiries

The cost of producing a chart or graph is comparable to that of printing out the data from the computer. A typical computer basic cost per chart might be 4 units (£5 to the general public at present), to which have to be added administration costs. The weight of paper is less than for a printout of the data it contains, and some customers may prefer to receive only the microfilm. The weather graphs can be inspected more quickly than can printouts, thus saving the customer time.

Enquiries about the charts and graphs from within the Meteorological Office should be addressed to Met 0 9, and from elsewhere to the Director-General, Meteorological Office (Met 0 3b), London Road, Bracknell, Berkshire RG12 2SZ.

### 4. Acknowledgement

The line drawers/chart plotters section of the Data Processing Branch wrote many of the programs which were used or modified to produce the charts and provided valuable assistance during the project.

551.589.6

## A note on singularities

By D. G. H. Battye

(Meteorological Office, Bracknell)

### Summary

The apparent tendency of short-period temperature fluctuations of like sign to recur at certain times in different years is investigated. Pseudo-random sequences of long-period mean daily temperatures are set up. The distributions of fluctuations over various time intervals are compared between random and real series. Differences are found to lack statistical significance.

### Introduction

From time to time, relatively large fluctuations of duration from 1 to about 10 days in period-mean daily temperatures at certain times of the year have excited interest because of their supposed predictive value, despite the absence of evident physical causes. This note compares such 'singularities' in a long-period series of mean daily Central England Temperatures (hereafter CET) with those in randomly generated equivalent series to determine whether they are likely to be chance variations due to sampling.

The value of singularities as noteworthy events in the climatological calendar is controversial. Brooks and Mirrlees (1930) noted that 'it has frequently been asserted that these warm and cold spells, or crests and troughs on the curve of temperature, are not strictly haphazard in their occurrence, but exhibit a definite tendency to cluster around certain days'. Lamb (1950) described a singularity as a 'typical short-lived but pronounced variation' and compiled a list of singularities through the year, although considering that their reality was not susceptible to testing by statistical methods.

McIntosh (1953) and Reynolds (1955) compared daily temperature series, from Edinburgh and Liverpool respectively, with harmonically smoothed curves. Their conclusions differed: McIntosh left the subject in doubt by considering such non-seasonal temperature variations to be very largely but not entirely random; Reynolds listed a series of 'warm and cold spells' which were thought to be

'worthy of publication', implying that they were notable singularities. Recently, from a more objective study, Aust (1979) has suggested that singularities are not all chance variations, but form an important part of the annual temperature progression.

### **The random-shift method**

The main difficulty in testing the statistical significance of temperature deviations from a smoothed curve lies in the nature of the data involved. Each sequence of daily means is autocorrelated to an extent varying with time of year and, more important, weather type. Thus the number of degrees of freedom in the final sequence of long-period mean daily values is itself variable, and not amenable to simple analysis. This note presents the results from an approach designed to overcome this problem. Original yearly sequences of daily temperature values are unaltered, but before the obtaining of sequences of long-period daily means, each year is advanced or retarded by up to 5 days or not moved at all, the precise adjustment being randomly determined. The character and degree of autocorrelation is thus preserved in the constituent 365-day sequences and hence also in the means. Real singularities in the actual series of long-period daily means would be expected to be removed or considerably reduced by this process, which is analogous to smoothing by an 11-point running mean. On the other hand, if further apparent singularities are generated in the new sequence of long-period pseudo-mean daily values, such that statistically the frequency distribution of small-scale variations is unaltered, then this constitutes strong evidence that the original singularities are themselves a feature of chance variation in the sampling.

### **Results of the random-shift method**

For this investigation 105 years of daily values of CET from 1871 to 1975 were used. Each daily temperature is one half of the sum of the daytime maximum and preceding night minimum averaged over a number of stations in England, the precise distribution varying with time; a full description is given by Jenkinson and Carr (1979). The curve of mean daily values through the year averaged from 1871 to 1975 is shown in Figure 1. Interesting apparent singularities can be seen at, for example, the cold period around 12 February, the warm period from 11 to 15 May and similarly the warmth near 15 December.

The 105 annual sequences of 365 daily temperatures were each subjected to a random time shift of between plus and minus five days, as already described. These were averaged to give pseudo-mean daily values through the year and this process was repeated 100 times. The frequency distribution was then found, over the complete set, of the temperature differences between all pairs of days from 1 to 10 days apart. The average distribution from these 100 runs of differences over periods of 1, 5 and 10 days is shown in Figure 2, together with frequencies of the sequence of actual mean daily values from 1871 to 1975. Because the simulated sequence of daily means is derived from 100 quasi-independent series, it may be regarded as chance expectation for the purposes of statistical testing. Table I gives values of chi-square ( $\chi^2$ ) for frequencies of temperature differences over all intervals from 1 to 10 days. It can be seen that only in the case of 5-day intervals is  $\chi^2$  significant at the 5 per cent level. This may well be a chance result when it is considered that 10 tests were carried out. Hence there is no evidence that 'singularities' in series of long-period daily means are other than chance superposition of similar temperature variations.

It may be of interest to see the scale of some generated 'singularities'. The examples of peaks, troughs and sharp gradients in Figure 3(a)–(f) were selected from the first 10 simulations. These may be compared with the actual sequence of daily means given in Figure 1.

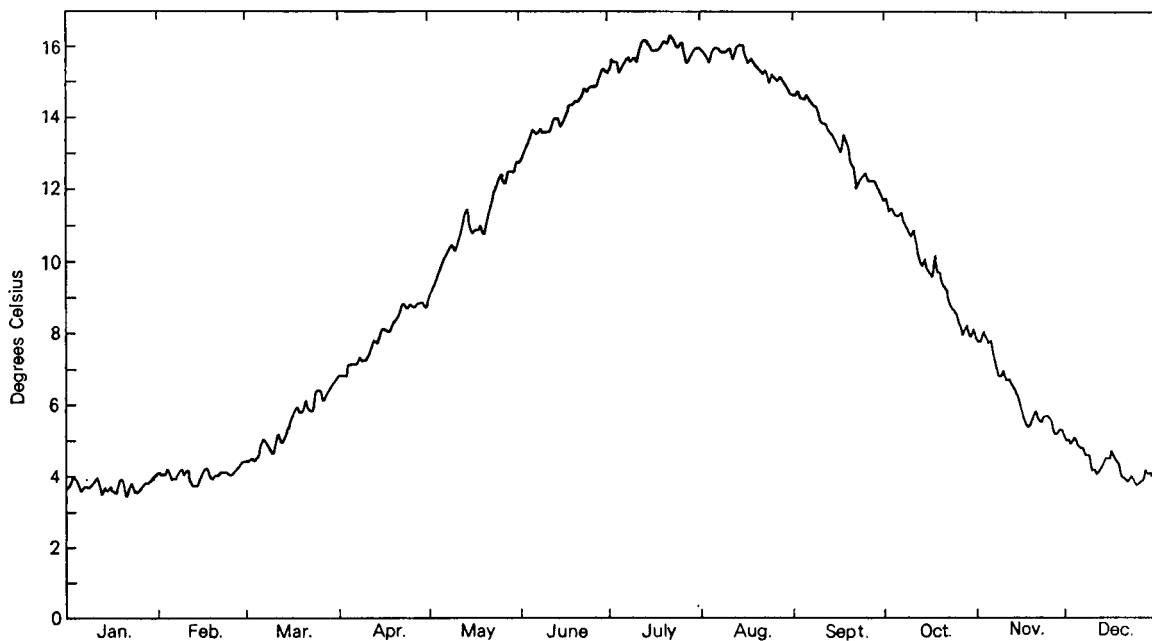


Figure 1. Mean daily CET, 1871-1975

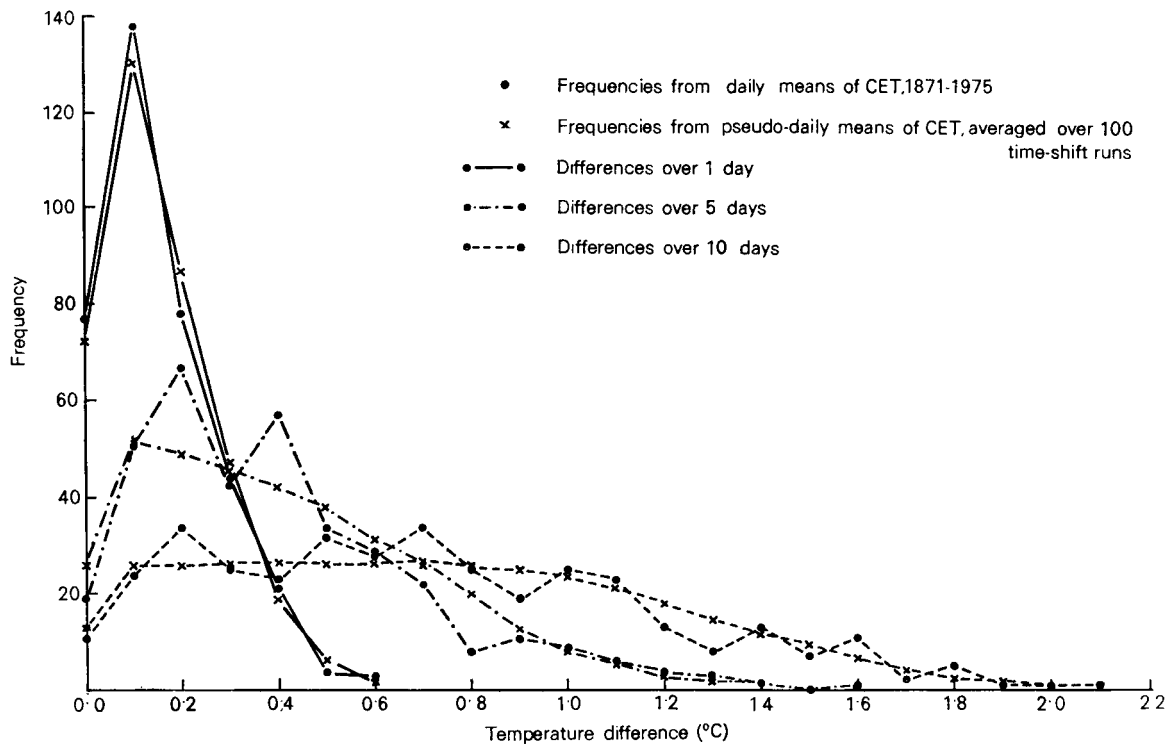


Figure 2. Frequencies of temperature differences at 1-, 5- and 10-day lags.



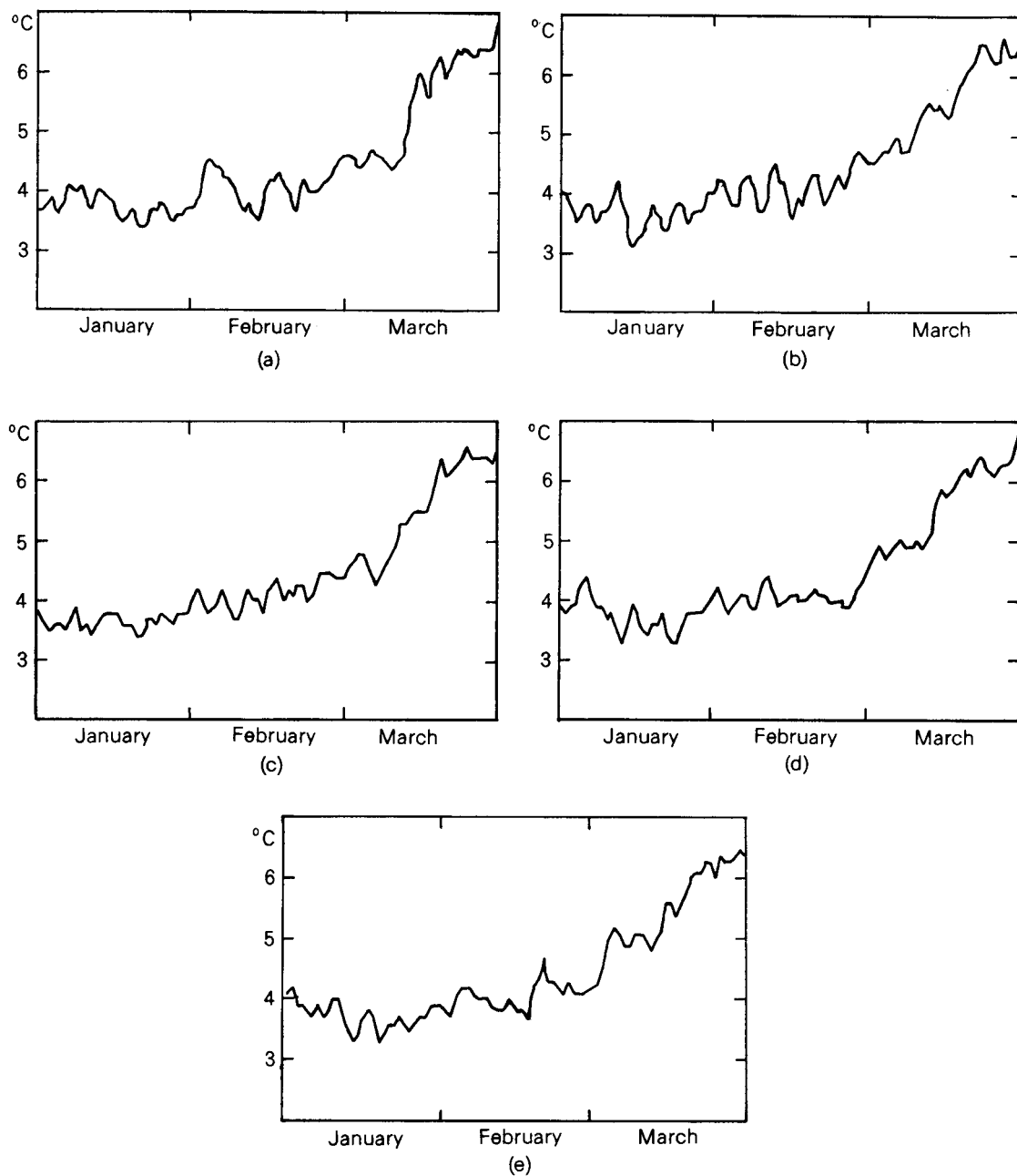


Figure 3. Examples of time-shift generated pseudo-mean daily CET series.

Table I.  $\chi^2$  values of temperature differences over 1 to 10 days

Lag (days)	Degrees of freedom	$\chi^2$ value	$\chi^2$ at 5 per cent significance level
1	5	2.4	11.1
2	7	13.8	14.1
3	9	15.0	16.9
4	11	16.4	19.7
5	12	23.9	21.0
6	13	10.6	22.4
7	14	12.3	23.7
8	15	12.0	25.0
9	16	13.1	26.3
10	17	16.8	27.6

The degrees of freedom are equal to the number of classes in the comparison, minus one.

### Conclusion

Sequences of daily values in the Central England Temperature series show no evidence of any consistent year-by-year variation on scales from 1 to 10 days that cannot be ascribed to seasonal trends or chance.

### Acknowledgements

Thanks are due to Mr R. N. Hardy for suggesting this approach and for advice at various stages.

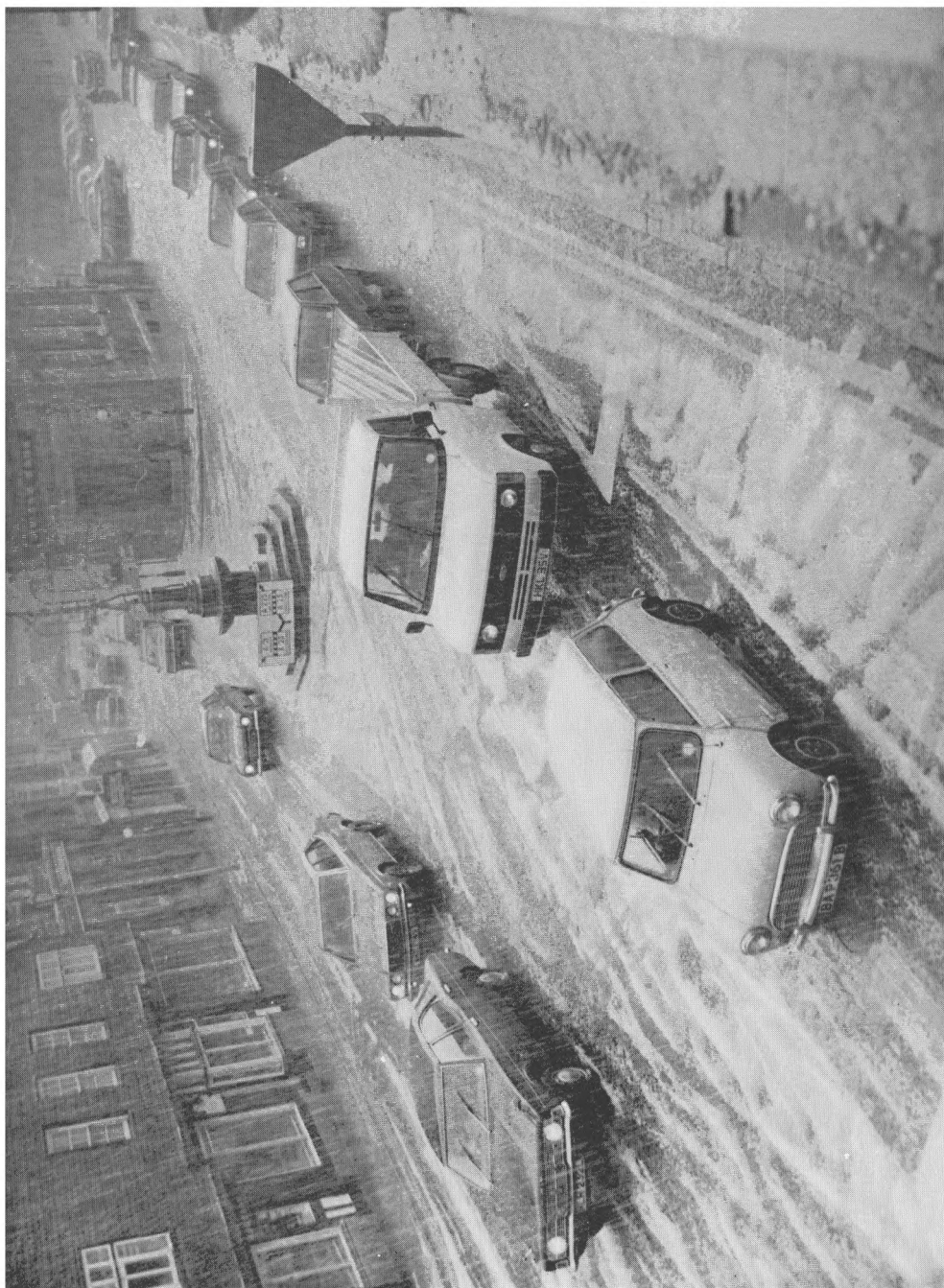
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## Letter to the Editor

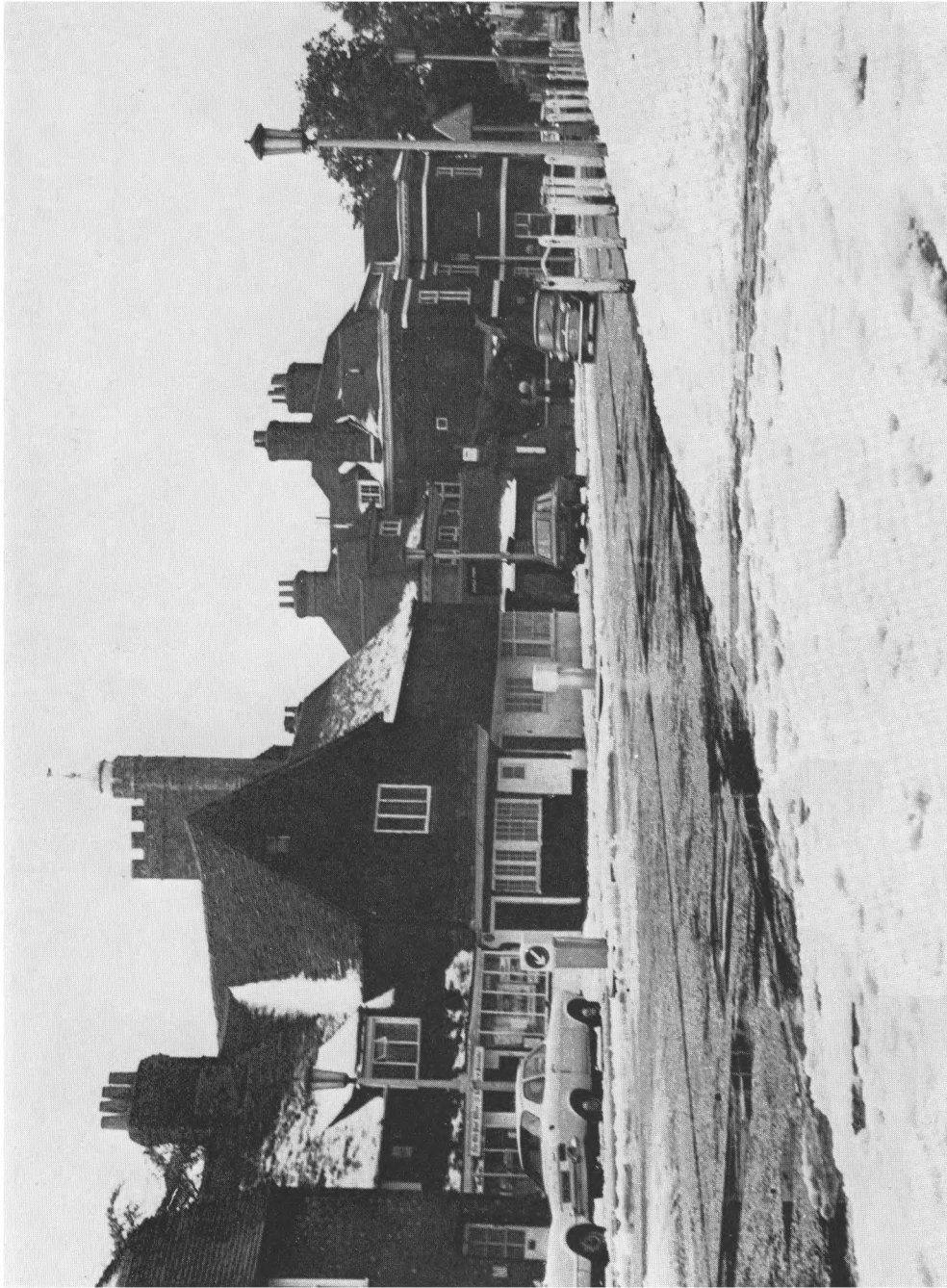
### Hailstorm at Sevenoaks, 26 June 1980

On 26 June 1980, a large shallow thundery low covered the British Isles and most of northern Europe. This low had approached from the north-west and proved very persistent and slow moving; its effects were felt for seven days. Thundery showers and storms had been experienced during the previous four days.



*Photograph by courtesy of Sevenoaks Chronicle*

Plate I. Hailstorm at Sevenoaks, Kent, 26 June 1980.



Photograph by courtesy of Sevenoaks Chronicle

Plate II. After the hailstorm at Sevenoaks.

June 26 dawned fine and sunny in the Sevenoaks area and these conditions lasted until early afternoon. Cumulonimbus development then occurred and a storm was observed to the south over the Tunbridge Wells area. However, it remained dry with sunny periods and reasonably warm at Sevenoaks until late afternoon.

The storm over Tunbridge Wells moved away and it appeared that the threat was over. However, there was a rapid development of cloud just to the east of the town from about 1530 BST. This was the herald of a remarkable and highly localized storm which began at 1630 and lasted until 1800. The sluggish wind and configuration of the land at Sevenoaks probably account for the intensity and localized nature of the storm.

At 1630 it began to rain, spasmodically at first and then steadily with increasing intensity. The accompanying thunder was of moderate intensity and continued so throughout the duration of the storm. The rainfall was quite another matter; it increased to a continuous torrential downpour the like of which I have experienced only in the Tyrol. The rain became interspersed with bursts of hail. These became more frequent until, in an area centred around Sevenoaks School, hail fell continuously for over half an hour with such intensity that the ground was covered to a depth of up to 6 inches with occasional much deeper patches. The area thus affected was about half a mile across. I measured several hailstones and found a diameter of a quarter of an inch several times.

As will be seen from the accompanying photographs the scene resembled a January snowscape rather than the aftermath of a June thunderstorm. The lying hail was still largely unmelted the next morning and some patches persisted in gullies etc. more than 48 hours later, despite temperatures of 15 °C.

The almost stationary nature and incredible intensity of the storm can be seen from the following readings of the rainfall between 1630 and 1815:

(1) Sevenoaks School	4.50 inches
(2) Knole Road (1 mile distant)	2.02 inches
(3) Cramptons Road (2½ miles distant)	0.84 inches
(4) Otford village (4 miles distant)	0.11 inches
(5) Shoreham village (6½ miles distant)	Nil.

The reading at (1) above will probably figure in the records of short-period intensity for Great Britain (see Ingrid Holford. *The Guinness book of weather facts and feats*, Enfield, Middlesex, Guinness Superlatives Ltd, 1977, p. 139). It rivals the Hampstead storm of 14 August 1975. At (2) above, the temperature was observed to fall from 17 °C to 6 °C in 30 minutes.

B. R. Dixon

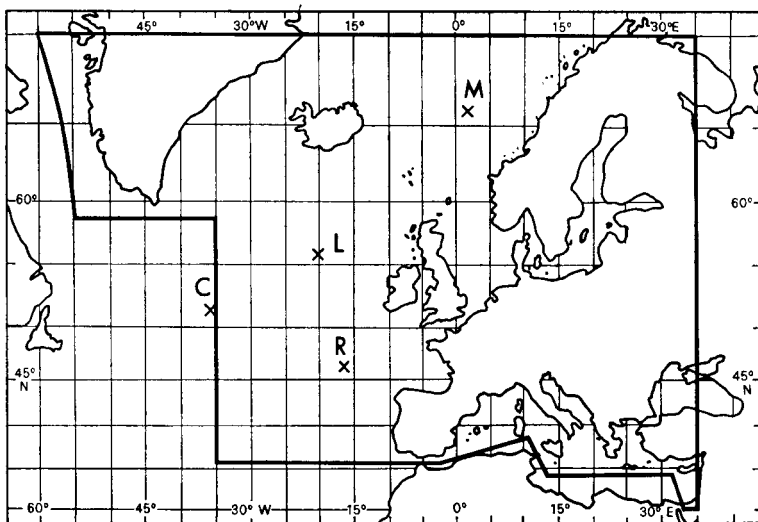
Sevenoaks, Kent.

## Notes and news

### Future plans for North Atlantic Ocean Stations

The fifth session of the Board established to administer the Agreement for Joint Financing of North Atlantic Ocean Stations met in Geneva in July 1980. The Board decided to maintain the present network beyond 1981, with some operational changes which might also reduce the network's present operating costs.

The most important operational change comprises a different way of manning the four ocean stations (see map). Furthermore, from 1982 onwards, the telex-over radio system of telecommunications will be used instead of the present Morse system.



Positions of the Ocean Stations in the NAOS network as from 1 January 1982. Station C is operated by the USSR and Station R is operated by France. Station L will be operated jointly by the United Kingdom and Netherlands and Station M will be operated by Norway.

The purpose of the Agreement, which was adopted on 15 November 1974 in Geneva by a Conference of Plenipotentiary Delegations, is to ensure the operation and financing of a network of four ocean stations in the North Atlantic for the purpose of making meteorological observations. Each station is permanently occupied by a ship specially equipped and staffed for carrying out surface and upper-air meteorological observations and for providing other services such as making oceanographic and other scientific observations and rendering safety services to other ships and to aircraft.

At present, the following 15 countries are parties to the Agreement:

Cuba	Norway
Denmark	Spain
Finland	Sweden
France	Tunisia
Germany, Federal Republic of	United Kingdom
Iceland	USSR
Ireland, Republic of	Yugoslavia
Netherlands	

A further six countries make annual voluntary contributions to the system.

WMO Press Release

### Long-range monthly forecasts—cessation of publication

As one of the measures to reduce costs and manpower in the Civil Service, the Meteorological Office has announced that it is to cease to issue long-range monthly forecasts at the end of the year. The publication of *Monthly Weather Survey and Prospects* (which contain the forecast) will also cease.

The service of forecasts for up to a week ahead—for example the farming forecasts, broadcast on a twice-weekly basis—where reliability has increased recently with the extension of computer-based methods, will be maintained. Some research on long-term prediction will continue, as will the existing consultancy service for specific customers.

The task of producing the monthly forecast is closely integrated with the normal forecasting and research activities of the Office, and it is not possible to be precise about its full cost. However, as a result of cessation of publication and reduction in associated research, seven posts will be given up with a resultant saving of approximately £47 000 a year at 1980 pay rates.

#### **Daily Weather Report—cessation of publication**

Publication of the *Daily Weather Report*, the *Monthly Supplement* to the *Daily Weather Report* and the *Daily Aerological Record* will cease at the end of this year.

This step is being taken as a result of the Government's determination to reduce public expenditure and the number of civil servants engaged upon work that, although desirable, is not absolutely essential.

Daily synoptic charts covering much the same area are available in the form of the *European Daily Weather Report*, prepared and distributed widely by the West German Meteorological Service, Deutscher Wetterdienst, Zentralamt D 6050, Frankfurter Strasse 135, Offenbach am Main, Federal Republic of Germany. The price is comparable with that of the *Daily Weather Report* of the Meteorological Office.

An historical account of the *Daily Weather Report* will be given in a future issue of the *Meteorological Magazine*.

#### **Symposium on Applications of Lidar to Atmospheric Radiation and Climate**

This symposium is to be held on 20 August 1981 in Hamburg, Federal Republic of Germany, and will form part of the IAMAP Third Scientific Assembly—Radiation Commission (17–28 August 1982). The co-convenors are:

V. E. Derr  
US Department of Commerce  
NOAA/ERL/WPL R45X3  
325 S. Broadway  
Boulder, Colorado 80303

V. E. Zuev, Director  
Institute of Atmospheric Optics  
Siberian Branch of the Academy of Sciences  
Tomsk-29  
Gertsena Street 8  
USSR

Abstracts are due by 2 March 1981 and should be sent to V. E. Derr.

#### **Meteorological Magazine—increase in price**

As from January 1981 the price of an issue of the *Meteorological Magazine* will be £1.80 and the annual subscription will be £23.80 including postage.

### **Licences for reception of meteorological broadcasts**

(This notice refers to broadcasts of technical data including radio-facsimile, not ordinary forecasts issued by public radio and television stations.)

The Radio Regulatory Department of the Home Office is responsible under the Wireless Telegraphy Act (1949) for the issue of licences for the reception of all telegraphy except sound broadcasting from authorized broadcasting stations or messages by telephony or telegraphy from licensed radio amateurs who have been exempted from licensing.

Licences for reception of meteorological broadcasts will be issued by the Radio Regulatory Department of the Home Office for an annual fee of £5 subject to the agreement of the Meteorological Office who, before giving consent, may levy an additional charge dependent on the use to be made of the data received. No additional charge will be levied on amateur meteorologists using the information for domestic and non-commercial purposes, on schools and colleges who use the information solely for instructional purposes or on most classes of shipping, provided that in each case an undertaking is given that the information will not be offered for resale in raw or processed form or otherwise be used in connection with the provision of any commercial service. In all other cases, the level of additional charge or waiver thereof will be decided in the light of information as to use given at the time of the licence application.

Any individual or organization wishing to be authorized to receive meteorological broadcasts from any source in the United Kingdom or in other countries should without delay address an application to:

Meteorological Office (Licensing)  
London Road  
Bracknell  
Berkshire RG12 2SZ.

The application, in letter form, should give details of all broadcasts to be received, reception frequencies, reception apparatus to be used and where this is kept, and the purpose for which the information received is to be used. The level of additional charge or waiver thereof will be notified to each applicant. After any payment due is received by the Meteorological Office, the application will be forwarded to:

Home Office  
Radio Regulatory Branch (R1)  
Waterloo Bridge House  
Waterloo Road  
London SE1 8UA

who will issue the licence on payment of the £5 fee.

### **Correction**

'A homogeneous rainfall record for the Cirencester area, 1844-1977' by P. D. Jones, *Meteorol Mag*, 109, 1980, 249-258.

The units quoted on page 256 in the heading to the table forming Appendix 2 should be 'tenths of a millimetre' not 'tens of millimetres'.





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December 1980

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## NOTICES

It is requested that all books for review and communications for the Editor be addressed to the Director-General, Meteorological Office, London Road, Bracknell, Berkshire RG12 2SZ, and marked 'For Meteorological Magazine'. The responsibility for facts and opinions expressed in the signed articles and letters published in this magazine rests with their respective authors.

Complete volumes of 'Meteorological Magazine' beginning with Volume 54 are now available in microfilm form from University Microfilms International, 18 Bedford Row, London WC1R 4EJ, England.

Full size reprints of out-of-print issues are obtainable from Johnson Reprint Co. Ltd., 24–28 Oval Road, London NW1 7DX, England.

Issues in Microfiche starting with Volume 58 may be obtained from Johnson Associates Inc., P.O. Box 1017, Greenwich, Conn. 06830, U.S.A.

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Printed in England by Heffers Printers Ltd, Cambridge  
and published by  
HER MAJESTY'S STATIONERY OFFICE

£1.60 monthly  
Dd 698260 K15 12/80

Annual subscription £21.18 including postage  
ISBN 0 11 722068 X  
ISSN 0026-1149